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SKY WITH OCEAN JOINED

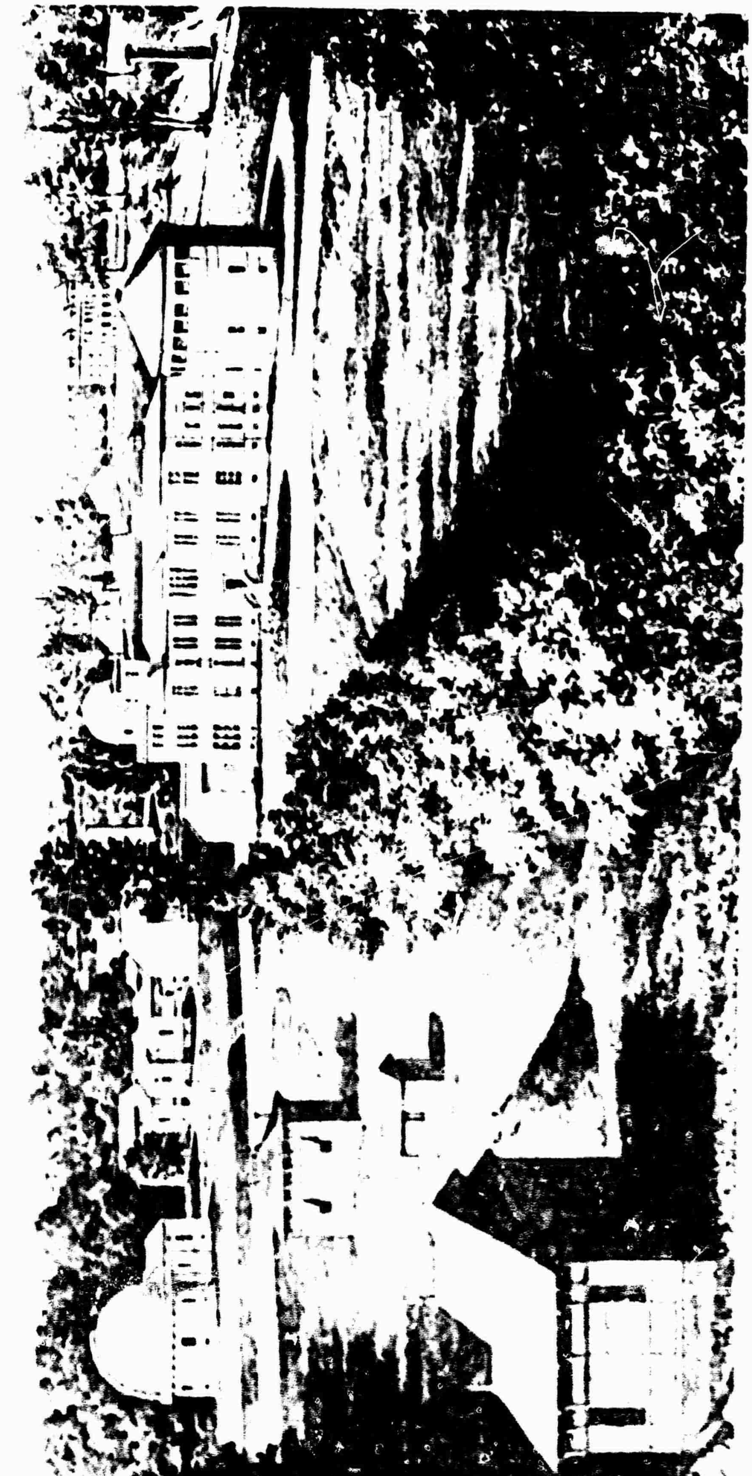
PROCEEDINGS OF THE

SESQUICENTENNIAL SYMPOSIA

U.S. NAVAL OBSERVATORY


U.S. NAVAL OBSERVATORY.
QUODDE GUBERNANDIS
STUDIUM PERVENIT IN ASTRA ET PONTUM
CAELO CONIUNXIT

SKY WITH OCEAN JOINED



SKY WITH OCEAN JOINED

PROCEEDINGS
OF THE
SESQUICENTENNIAL SYMPOSIA
OF THE
U.S. NAVAL OBSERVATORY
DECEMBER 5 and 8, 1980

Edited by
STEVEN J. DICK
and
LeROY E. DOGGETT

U.S. NAVAL OBSERVATORY
WASHINGTON, D.C.
1983

Inside covers: The seal of the U. S. Naval Observatory show Urania reaching toward the stars, with the terrestrial globe in her outstretched hands. The motto, taken from the Fourth Book of the *Astronomicon* of Manlius, may be translated,

Then, too, the pilot's care: the stars are sealed
And sky with ocean joined

Both the seal and motto emphasize the Observatory's origin in the need for improved navigation. See [C. H. Davis], "[illegible] Seal of the U. S. Naval Observatory," *Astronomical and Meteorological Observations Made at the U. S. Naval Observatory During the Year 1865* (Washington, 1867).

Frontispiece: Artist's sketch of the present site of the Observatory, made shortly after construction was completed in 1894.

This book is set in Aldine Roman type.

PREFACE

If historians of science debate whether there is a distinctively American science, there is little doubt that there are distinctively American scientific institutions. Influenced not only by a century and a half of science and technology, but also by national, regional and local events, the U. S. Naval Observatory is among the oldest of these institutions.

The occasion in December 1980 of the 150th anniversary of the founding of the Depot of Charts and Instruments, forerunner of the Naval Observatory, provided an opportunity to reflect on the Observatory's past, present and future. It was also an opportunity for celebration. Through the efforts of the Observatory's Public Affairs Committee and the Committee on the Preservation of Historic Instrumentation, Photographs and Documents (familiarly known as the History Committee), a wide variety of celebratory activities was organized. There was a conscious attempt to present the institution in turn to the Navy, the academic community and the public, and to Observatory personnel, current and past. On the morning of Friday, December 5, an official ceremony was held in the Library. The program of speakers is printed on page viii. In the afternoon the historical symposium was held. And in the evening there was a party for staff and friends of the Observatory. Saturday was devoted to a day long open house for the public. The final event, on Monday morning, was the symposium on current work and future goals.

There are many ways to measure the success of the festivities: the list of distinguished speakers, the quality of the presentations (as documented by this volume), the number of visitors, the exhaustion of the organizers. The open house on Saturday, December 6, was overwhelmingly successful. Encouraged by clement weather, more than 6100 people visited the Observatory, viewing the special exhibits and facilities, from telescopes to atomic clocks. The National Capital Astronomers assisted by bringing their own exhibits and a telescope for solar observations. At 11 a.m. the sounds of bells were heard as the Washington Cathedral's Ringing Society began a commemorative quarter peal of Morning Star Treble Bob Minor. This was made possible by Ms Nevile Withington, who is a member of both the Observatory staff and the Ringing Society.

In a more scholarly vein, the two symposia reflected the two [illegible]

the Observatory's life: a rich history which thus far has received little treatment,[1] and the ongoing scientific programs, which result in publications and data used by the government, the Navy, the international scientific community, and the general public.[2] All of the papers appearing herein are based on transcripts of tapes of the symposia, and have been altered only to reflect the not always trivial differences between the spoken and written word. The only exception is the paper that appears in the Appendix, which was not part of the symposia but was written on the occasion of the sesquicentennial for *Sky and Telescope*.

Taken together, it is hoped that these papers will represent a small contribution to a badly needed history of American scientific institutions, as well as help to fulfill the no less pressing need for public understanding of the work of modern scientific institutions.

Thanks are due to the members of the Observatory's Public Affairs Committee[3] for their help in organizing the anniversary events, to the members of the Committee for the Preservation of Historic Instrumentation, Photographs and Documents[4] for their help in proofreading this volume, to Marion Sosslau for help in composing the typescript, and to F. R. Rafferty, R. E. Schmidt and M. Miranian for their photographic work. Finally thanks must go to Captain Raymond A. Vohden, USN, and Commander John L. Hanna III, USN, respectively the Superintendent and Deputy Superintendent of the Naval Observatory during the anniversary, and to Gart Westerhout, P. K. Seidelmann and all the symposia participants, whose cooperation made the symposia and this volume of *Proceedings* possible.

S.J.D. & L.E.D.
September 1983

NOTES

1. The major exceptions are the aging monographs by J. E. Nourse, *Memoir of the Founding and Progress of the United States Naval Observatory* (Washington, 1873), and Gustavus A. Weber, *The Naval Observatory: Its History, Activities and Organization* (Baltimore, 1926). Arthur L. Norberg, *Simon Newcomb and Nineteenth Century Positional Astronomy* (Ph.D. dissertation, University of Wisconsin, 1974), is closely related to one facet of Naval Observatory history. Other aspects of the history are discussed by Edgar W. Woolard, "The Centennial of the American Nautical Almanac Office," *Sky and Telescope*, 11 (December 1951): 27-29; Owen Gingerich, "The Satellites of Mars: Prediction and Discovery," *Journal for the History of Astronomy*, 1 (1970), 109-115; R. W. Rhynsburger, "A Historic Refractor's 100th Anniversary," *Sky and Telescope*, 46 (October 1973), 2-8; P. M. Janiczek and L. Houchins, "Transits of Venus and the American Expedition of 1874," *Sky and Telescope*, 48 (December 1974), 366-371; Arthur L. Norberg, "Simon Newcomb's Early Astronomical Career," *Isis*, 69 (June 1978), 209-225; Howard Plotkin, "Astronomers versus the Navy: the Revolt of American Astronomers over the Management of the United States Naval Observatory," *Proceedings of the American Philosophical Society*, 122 (December 1978), 385-399; John Lankford, "A Note on T. J. J. See's Observation of Craters on Mercury," *Journal for the History of Astronomy*, 11 (1980), 129-131; Steven J. Dick, "How the U. S. Naval Observatory Began, 1830-65," *Sky and Telescope*, 60 (December 1980), 466-467, reprinted in this volume; [illegible] Bartky and Steven J. Dick, "The First North American Time Ball," *[illegible] History of Astronomy*, 13 (198[illegible]), [illegible]; Jack K. Herman, "Home of [illegible] MED: [illegible] Foggy Bottom," *U. S. Navy Medicine* (February, March, April, [illegible], July, September 1982). The last title refers to the Navy's Bureau of Medicine and Surgery, now located at the site of the old Naval Observatory.

2. Some of these publications are discussed in the papers by the Director [illegible].

3. Philip [illegible] A[illegible], Sally [illegible] B[illegible], Brenda [illegible] C[illegible], [illegible] Virginia B. F[illegible], [illegible] Stephen [illegible], CDR John [illegible] [illegible] III USN, [illegible] Jennifer [illegible] Ramsey, Carl Westerhout.

4. Gail S. Cleere, Brenda G. Corbin, Thomas E. Corbin, Steven J. Dick, [illegible] [illegible]ett, Alan D. Fiala, Carl E. Fokas, Marie R. F[illegible], Theodore [illegible] [illegible] Schmidt, Rosa E. Trantham, E. Nevill Withington

SESQUICENTENNIAL CEREMONY

U. S. NAVAL OBSERVATORY

December 5, 1980

Welcoming Remarks

RADM R. N. Williams, Oceanographer of the Navy

CAPT R. A. Vohden, Superintendent, U. S. Naval Observatory

Brief Remarks

Mr. R. A. Frank, Administrator, National Oceanic and Atmospheric Administration

RADM A. J. Baciocco, Jr., Chief of Naval Research

Dr. T. Calio, Associate Administrator, National Aeronautics and Space Administration

RADM E. A. Wilkinson, Jr., Deputy Director, Defense Mapping Agency

Dr. E. Ambler, Director, National Bureau of Standards

Dr. R. Berendzen, President, The American University

Dr. H. Friedman, Chairman, Division of Mathematical and Physical Sciences, National Academy of Sciences

Dr. F. E. Johnson, Assistant Director for Astronomical, Atmospheric, Earth and Ocean Sciences, National Science Foundation

Dr. N. Hinners, Director, National Air and Space Museum, Smithsonian Institution

Dr. G. Westerhout, Scientific Director, U. S. Naval Observatory

CONTENTS

NAVAL OBSERVATORY TIME DISSEMINATION BEFORE THE WIRELESS

Ian R. Bartky
National Bureau of Standards

When I began to prepare for this talk, I thought it would be a straight forward matter: I would discuss quality work by a fine institution. As I began to dig a bit into your history a history that except for Steve's article[1] is almost unpublished I found that there were a number of incidents which would indicate a *great* institution. One of your founders was court martialed twice before he retired as an admiral; one of your astronomers was banned from the journals and exiled to California for the rest of his career. And so I concluded that we had here an institution of unusual people; we had controversy, and thus an environment existed for creative work.

Now, the second thing I noted was that in this area of time dissemination, the Naval Observatory had done a tremendous number of things. For today I have grouped them into five areas: Longitude, Controlled Clocks, Public Time, Uniform Time, and [illegible] three-hour symposium, but they are not giving me all the time, [illegible] will have to go over these areas rather rapidly.

We are talking about time *dissemination*. The job of *determining* the time . . . well, that is up to the astronomers. We will start, let's say, at the door of the observatory. The time has been determined, is being kept some place, and we want to give it to some particular group.

Longitude

The telegraphic determination of longitude was perfected in the United States, and this process is perhaps the first use of disseminated time in the United States. What you do is exchange and compare clock signals. The first date given in Table 1 is usually listed as the third one in the history of telegraphic determinations of longitude. The first and third determinations shown were measured on the experimental line of Samuel Morse between Washington and Baltimore. The name associated with the first date [illegible] friend Admiral Charles Wilkes, whose two court martials were part of a career that included nearly getting us into a war with Great Britain [illegible]

Table 1. *Time Dissemination for Longitude*

June 1844	Washington	Baltimore	Wilkes
1845/1846	Part of Observatory plans		Maury, Vols. I, II
Summer 1846	"Wires connecteing the main lines at Washington with the Washington Observatory . . . set up by the U. S. Coast Survey."		
Oct. 1846	Washington	Philadelphia	S. Walker
Summer 1847	Washington	Jersey City	S. Walker
1849	Washington	Cambridge	Coast Survey

Civil War.) The second and third dates in this table are not listed in the histories. As you see, the first Superintendent, Matthew Maury, indicated that the telegraphic determination of longitude would be part of the Observatory plans. The next two determinations (October 1846 and summer of 1847) were performed by the use of the first commercial telegraph line, the line between Washington and New York City. I want to show with the last date (1849) that very early on Washington was tied in to the zero of the longitude net of the United States, which was Cambridge because of exchanges of chronometers with Greenwich. Your history gives many more of these longitude-difference determinations between Washington and other places, but I wanted to show with this series of dates that the Naval Observatory was doing state-of-the-art work in this area from the first.

Table 2. *Controlled Clocks*

1868	Navy Department
1871	Army Signal Office
May 1873	Treasury Department
1873	Western Union, Washington Office
1877	Sate, War and Treasury
1880	Automatic transmission of time
1884	Gardner (USNO) System:
	Executive Mansion
	Senate
	National Museum
	Smithsonian Institution
	Others
1885	Gardner clocks: from 20 to 84
1886	200 Gardner clocks on clock circuit
1887	[illegible] clocks controlled by USNO time line
[illegible]	System discontinued

Controlled Clocks

When you dial up the Naval Observatory Time Service, you hear about the master clock. Well, obviously if there is a master clock, there must be slave clocks. So I dug a bit into the history to see at what stages you were controlling clocks in various other parts of Washington, and I show in Table 2 the Navy Department, the Signal Office, and so on. It becomes rather interesting in 1880 when you are able to give out time ticks along your telegraph line automatically. And so in 1884 we have this rather large clock system, which is a clock line from a mean time clock at the Naval Observatory controlling clocks in government buildings in Washington.

We should say something about the mention of William F. Gardner in Table 2. He is your instrument maker at the Observatory from 1865 to 1898. For those of us who are experimentalists, it is this sort of person who makes laboratories run. Now, Gardner has six patents on these clock systems, clocks which he can adjust automatically from the Naval Observatory. I really wanted to show you a picture of one of the clocks, but what I show is about all that is known about this particular time dissemination system of the Naval Observatory.

Public Time

The Naval Observatory is also giving out time to the general public. This, too, starts rather early on as you see from Table 3. The Naval Observatory is connected in 1865 by a telegraph line to the Washington Fire Alarm Office and rings the fire bells three times during the day. The branch circuit listed

Table 3. *Public Time*

Aug. 1865	Washington fire bells (7 a.m., 12 noon, 6 p.m.).
1865	To State (Orphan Asylum) via fire bell circuit
~1865	Western Union branch to main office.
1869	Western Union loop from Observatory, then to B&O, Southern RR
March 1871	USNO to Western Union requesting time distributed for science (e.g., Smithsonian observers)
1871	Army Signal Corps system of Observatory time expanded to cover the U.S. for meteorological stations.
Dec. 1876	USNO to Western Union for "spread of Standard Time." Proposes creation of time service to cities > 20,000
March 1877	USNO prepares plans for NYC time ball for Western Union.
April 1877	Western Union announces new time service. Washington Time to all points
10 Sept. 1877	USNO drops NYC time ball (New York Time).
~1881	Several Washington horological institutes on line to USNO

ran from the Fire Alarm Office to the State Department, and they, too, had asked for a time signal in that year.

In the Orphan Asylum, the building that housed the State Department, there was also a branch office of Western Union. Western Union looped into this particular signal system and brought the time signals from there to their main office in Washington. We are not sure, but we think that they probably distributed it throughout the United States. What we are sure of is that some time before 1869 there was a direct telegraphic loop from the Naval Observatory to Western Union, and those time signals were distributed to the Baltimore & Ohio Railroad and to southern railways.

The 1871 date is a very interesting one because it shows the start of a very close relationship between the Naval Observatory and Western Union. In 1871 the Naval Observatory asked Western Union to provide Washington time to the Smithsonian meteorological volunteers. Also, in that year the Signal Corps' clock, which was connected to the Observatory along another line, was expanded as a system for the military meteorological stations.

The 1876 date in Table 3 is very interesting, too, because it is at this time that the Naval Observatory suggests to Western Union that they provide a new time service: the distribution of Washington time to any city with a population of 20,000 or larger. We are going to be talking about time balls a little later, but I thought it was interesting that during these negotiations, the plans for the Western Union time ball in New York City were developed by the Naval Observatory. In April 1877 Western Union announces this time service, and the New York City time ball is dropped in September. This era is an exciting one for people: they are seeing the uses of electricity, they are seeing the availability of accurate time. Public interest is illustrated by a cut from *Harper's Monthly* of 1878 (Figure 1).

Uniform Time

This was also the era of Standard Time, and some important dates are shown in Table 4. Previous to 1883, as many of you know, cities kept local

Table 4. *Uniform Time*

18 Nov. 1883	Standard railway time in U.S.; USNO retards NYC time ball
1 March 1884	Washington on 75th meridian time.
1884	Mare Island time distribution for U.S. west of Rockies
1890	22 observatories complain re USNO Western Union
1908	Telegraphic signals to Mare Island 0.3s early; new longitude determination.
1918-1919	U.S. Standard Time zones, DST
19[illegible]6	Western Union terminates use of Mare Island time

Fig. 1. From *Harper's New Monthly Magazine*, 56 (1878), 665-71.

time, the time of their local meridian. The railways changed that. We start the list with Standard Time for the railroads; Washington stayed on its own local time until the following March. I want to mention in passing that not only was time being distributed from Washington, but also time was being distributed from the Mare Island Naval Shipyard, north of San Francisco. Let me show you a picture (Figure 2) that probably very few of you have

Fig. 2. U.S. Naval Observatory at Mare Island Naval Shipyard, California. From S. Lemmon and E. Wichels, *Sidewheelers to Nuclear Power*, Annapolis, Leeward Publications, 1977. Reprinted with permission.

seen. This is the U. S. Naval Observatory at Mare Island with T. J. J. See, your astronomer-in-exile, standing in front. His tour of duty there was longer than anyone else's.

The need for accurate time was a very important one for cities and for railroads, and one of the ways astronomical observatories made money for astronomical research was to sell time to them. There is a paper I have seen in the files of the Smithsonian that says when Langley was director of the Allegheny Observatory he brought in $66,000 as a result of selling time to the cities and railroads. So, needless to say, in 1890 many observatories were rather unhappy about the close relationship between the Naval Observatory and Western Union. I have yet to see in the Archives the actual complaint to the Secretary of the Navy, but I have seen the answer. It goes something like this: the determination of time is relatively inexpensive; it is the distribution of time that is an expensive proposition. Obviously, Western Union has the telegraph lines, and obviously they can refuse their use to all the observatories, and obviously we end up with uniform time. This means, not time from many recognized authorities such as the 22 observatories that complained, but time from one place—the Naval Observatory.

I think of uniform time, in the sense of time from one place, as starting in 1926 when Western Union terminated the use of Mare Island time. And, as you can see from Table 4, in 1908 time from there was obviously a little bit different from the time in Washington.

Time Balls

We are going to see a lot of pictures and there is a great deal of material here. A large number of people were most helpful in acquiring the material, and I must mention Brenda Corbin of the Naval Observatory Library, Dr. Steven Dick of the Observatory staff, Marc Pinsel of the Naval Oceanographic Office in Bay St. Louis, and, of course, Dr. Sharon Gibbs of the National Archives.

Time balls—what were they for? From *The American Practical Navigator* (1900) of the time:

> In certain ports which are in telegraphic communication with the principal observatory, a time ball is dropped . . . and by comparing with it the chronometer reading . . . the error as well as the rate may be established.

You see that a time ball is for checking one's chronometer for sea navigation. What was it? There was a ball on a staff (Figure [illegible]), and someone [illegible]

it. Someone else was watching the clock and, at a particular time, he dropped the ball. In England and her colonies, the time ball was dropped at one o'clock. In the United States, it was dropped at noon. Where were they? Where were the first ones? Table 5 shows that one was at Greenwich in 1833.[2] The next one is on the island of St. Helena in the Atlantic Ocean. The Naval Observatory's time ball is also a very early one. Telegraph Hill in San Francisco is in 1852. The next two I list are very famous ones, and the last one in the table is the first time ball in Canada.

Let's go to the North American time balls, which are listed in Table 6. We have done a lot of digging to find out exactly when the Naval Observatory dropped its first time ball. But you see there are six dates, and we really cannot determine which one it is. Based on what we know so far, it looks like 1855, and that says it was not the first time ball in North America, unfortunately, but it might have been the third one.[3] The 1877 date is the time ball in New York that the Naval Observatory dropped by direct telegraphic signal from Washington. The last date is Harvard College Observatory dropping the Boston time ball.

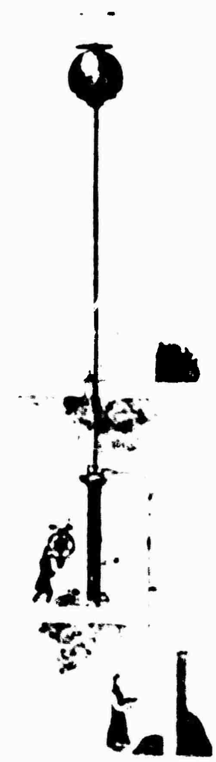


Fig. 3. From *Illustrated London Almanack for 1845*, p. 28

Figure 4 is a bit of a historical find for us. Steve Dick had a date for something about time balls in the Archives of the Secretary of the Navy, and when I looked at it I realized what it was. It is the idea for the first time ball. The inventor was Captain Robert Wauchope of the Royal Navy. His idea was

Table 5. *The First Time Balls*

28 Oct. 1833	Royal Observatory, Greenwich
Dec. 1834	St. Helena
. . .	. . .
184?	National Observatory, Washington
. . .	. . .
May 1852	Telegraph Hill, San Francisco
Aug. 1852	The Strand, London
1855	Navy Yard at Deal, England
1855	Quebec, Citadel at Cape Diamond

Table 6. *North American Time Balls*

"Early Forties"	
1844	
1845	National Observatory, Washington
>7 Feb. 1845?	
<1849	
1855	
May 1852	San Francisco (City Observatory)
1855	Québec (Observatory at Citadel)
1859/61	Albany, NYC (Dudley Observatory)
1862/63	Montréal (McGill Observatory)
March 1870	Fort Howe, Saint John, N.B.
<1871	?? Toronto (Magnetic Observatory)
1871?	Cincinnati Observatory
10 Sept. 1877	New York (USNO Telegraphic Signal)
1878/81	St. Louis (?)
8 May 1879	Boston (Harvard College Observatory)

to have two balls one is fixed and you drop the other one at a particular time, and so on. The material is six years earlier than any of the dates given in histories would indicate, so quite obviously it is something we're a little excited about, and we will write something up on this particular find."

Now we will show you some pictures. Figure 5 is Greenwich in 18[illegible] with the time ball displayed prominently. Figure 6 is the only evidence for a starting date earlier than 1855 for the Naval Observatory time ball: a picture taken from Volume 1 of the *Washington Observations* of Maury (18[illegible]). We do know from information in 1881 how it was dropped. It fell to the dome of the Observatory and then rolled down onto the roof. It was not sliding down a staff.

Figure 7 is given simply to show a nice looking time ball in Canada. The ball in cage is an idea for trying to get it to drop a little better in [illegible] time. I do not know very much about the time ball at the Custom House in New York City shown in Figure 8. But I know about the one in Figure 9. It is the New York City time ball the Naval Observatory dropped by a telegraphic signal. The illustration is from *Scientific American* of 18[illegible].

Figure 10 shows the Boston time ball that was dropped by hand from the clock signal at the Harvard College Observatory. That particular ball was four feet in diameter and weighed 400 pounds. The accuracies that were claimed here were 0.2 1.0 second, and that was certainly sufficient for sea navigation purposes.

Figure 11 shows locations of the U. S. time balls for sea navigation. The three dots on the West Coast show those dropped by direct [illegible]

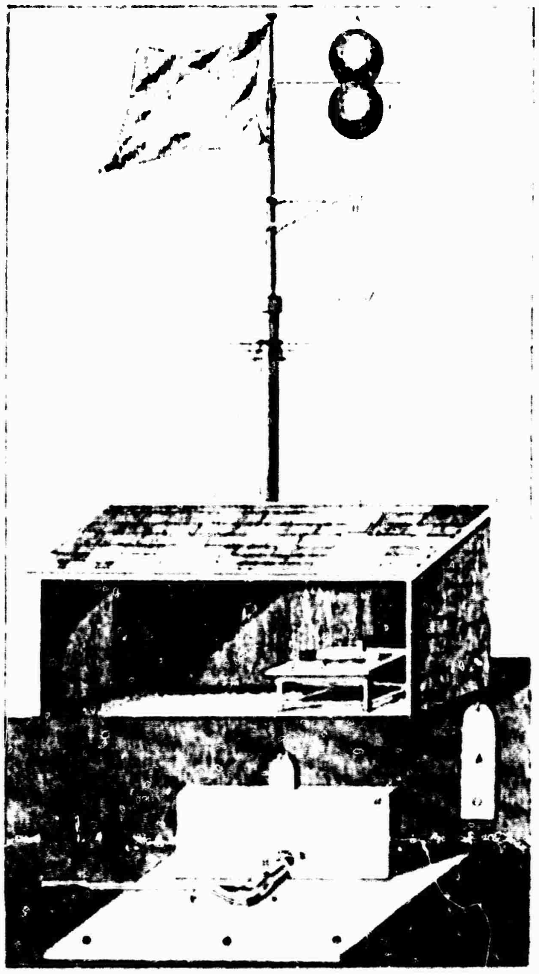


Fig. 4. Plan of the first time ball. From the National Archives, Record Group 45: Naval Records Collection of the Office of Naval Records and Library; Entry 222: Records of the Board of Navy Commissioners. Letters Received from the Secretary of the Navy, v. 1829-30.

Fig. 5. Greenwich Observatory, [illegible]. With permission, National Maritime Museum, London.

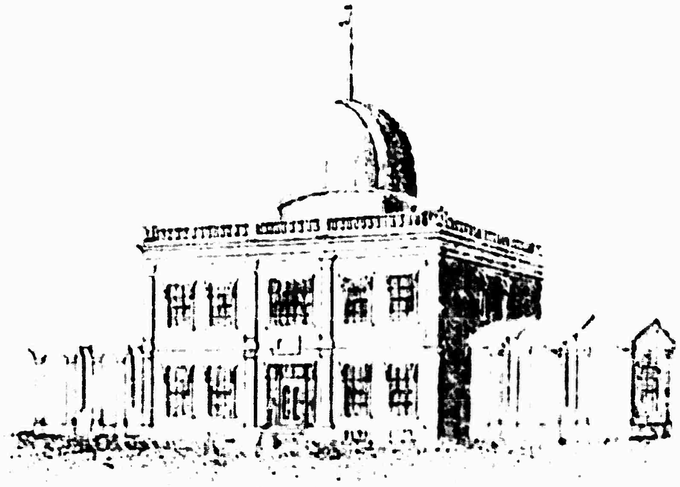


Fig. 6. Earliest evidence of a time ball at the Naval Observatory. From *Astronomical Observations (1845) of the U. S. Naval Observatory*. Washington: J. & G. S. Gideon, 1846.

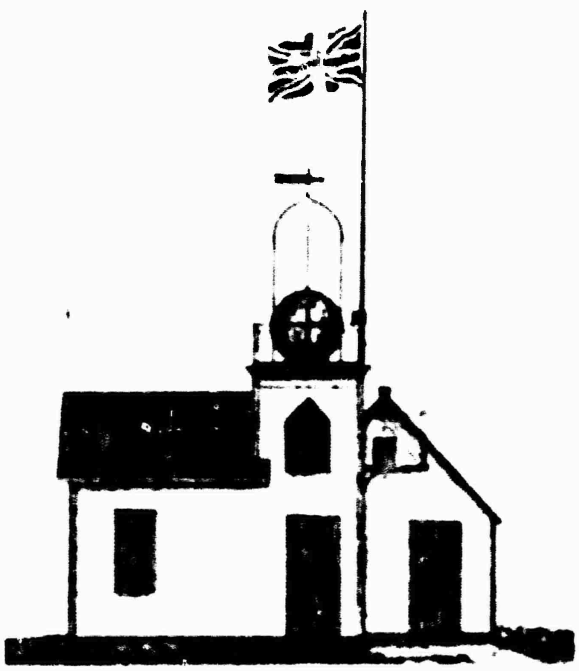


Fig. 7. Fort, St. Johns, New Brunswick
Courtesy, New Brunswick Museum

Fig. 8. Time ball on the old New York Custom House. From *Harper's New Monthly Magazine*, 56 (1878), 665-61

Fig. 9. New York time ball controlled by Naval Observatory. From *Scientific American*, 39 (1878), 335, 337.

Fig. 10. Boston time ball. From War Department, Professional Paper No. 5 of the Signal Service. Washington: Government Printing Office, 1881.

Figure 11. U. S. time balls for sea navigation prepared 11 80

from Mare Island. By this I mean: we raise the ball, hold it up with an electromagnet, and then a telegraphic signal drops the ball precisely at noon time. Some of the ones on the East and Gulf Coasts were dropped by direct telegraphic signals from the Naval Observatory at Washinton. Those shown pretty much in the Central Time Zone were dropped by branch Hydrographic Offices. They got the Observatory's time signal at eleven o'clock their time. They had their own clocks, and so they dropped the time ball an hour later. The dot on Detroit is one that I found only in a German document, and in no other list. Neither the Naval Observatory nor the branch offices had anything to do with that particular time ball.

The cities given in Figure 12 are the same as on the preceding map. What I want to highlight is that the heyday for time balls began about 1880 and went to 1925. That is when radio time signals became important. If you can get time at sea you really are no longer interested in getting time only when you are at a seaport.

Now I am going to go through a number of specific time balls. We will have a few sequences, and then we will be finished with this talk.

We will start with San Francisco. Figure 13 is a picture from 1880 showing Telegraph Hill. The structure on the Hill is shown in more detail in Figure 14, where you can see the time ball on the top. There is a date for the photograph of 1882, but we know that date is wrong because San Fran

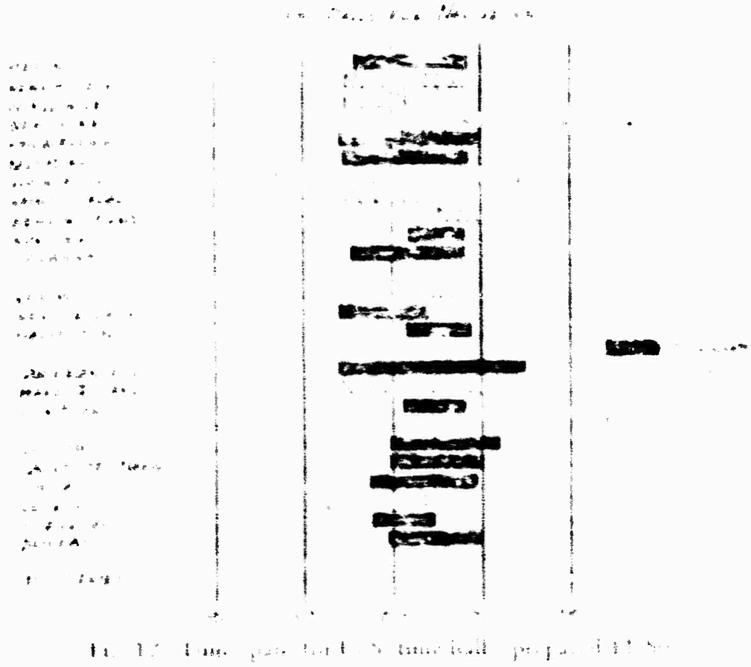


Fig. 12. Time span for U.S. time balls, prepared [illegible]

Fig. 13. Telegraph Hill, San Francisco. From California Historical Society/San Francisco, with permission.

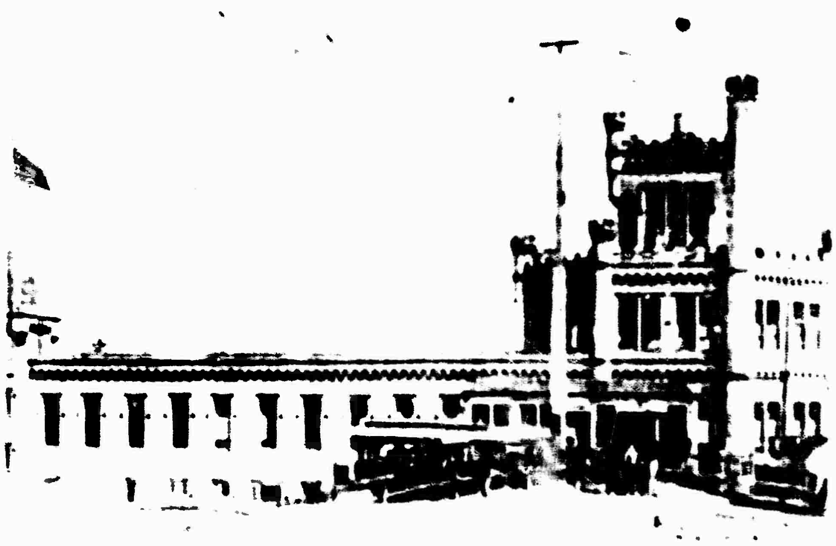


Fig. 14. Telegraph Hill, San Francisco. From California Historical Society/San Francisco, with permission.

cisco's time ball was controlled by the branch Hydrographic Office, and it didn't start until after March 1885. The new Ferry Building was built in 1898, and Figure 15 is a picture from that era. The branch Hydrographic Office put the time ball at the top. In 1909 they switched to the Fairmont Hotel, and Figure 16 shows the time ball on top of it.

We all know the building in Figure 17. We call it the Old Executive Office Building now. It was the State, War and Navy Building. Figure 18 is a picture of the time ball on the roof. This time ball was in use until 1936. The ball slid down into a retaining tub (Figure 19), and you don't see it except around noontime. Dropping it in leads to problems, of course. When the tub is iced over, you really cannot raise it and give a signal that day. The ropes to raise it broke occasionally. Sometimes you lose the time signal from the Observatory. Sometimes you forget the key to the attic, so you are unable to raise it that day. The failure rate was 5.8%.

Figure 20 pertains to our friend Mr. Gardner again. He has two patents on time balls alone. What that really means is that they start looking very much alike. Figure 21 shows

Fig. 15. Ferry Building, San Francisco. From California Historical Society/San Francisco, with permission.

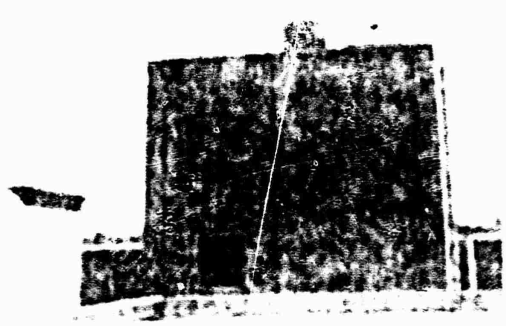


Fig. 16. Fairmont Hotel, San Francisco. From National Archives Record Group 37, [illegible] June 2, 1909.

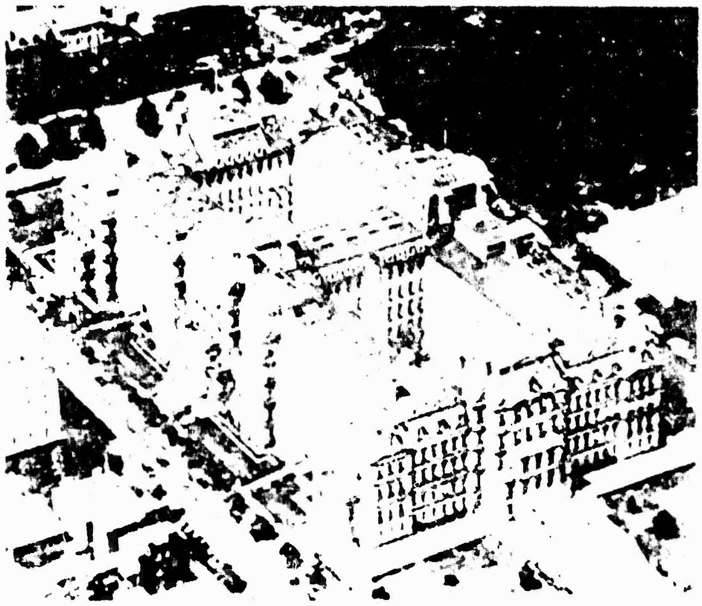


Fig. 17. From *Executive Office Building; General Services Administration Historical Study No. 3*. Washington: Government Printing Office, 1970.

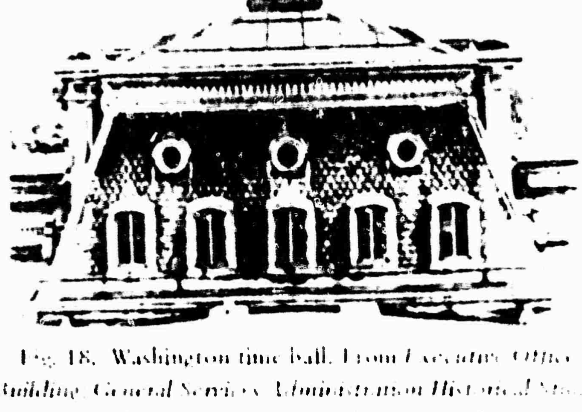


Fig. 18. Washington time ball. From *Executive Office Building; General Services Administration Historical Study No. 3*. Washington: Government Printing Office, [illegible]

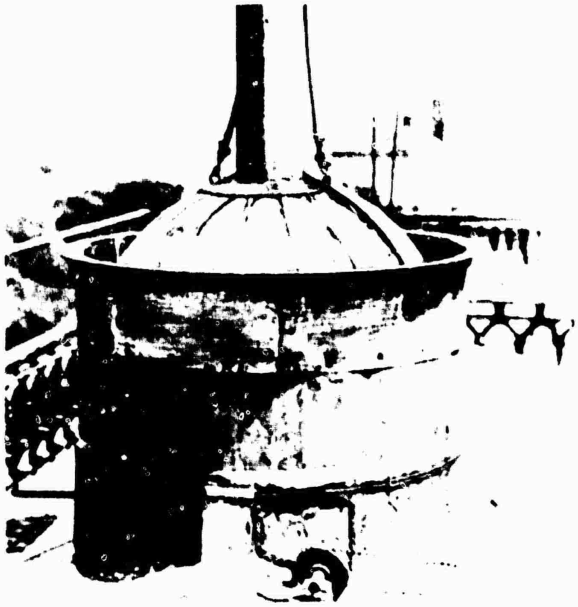


Fig. 19. Time ball retaining tub. From *Executive Office Building: General Services Administration Historical Study No. 3.* Washington: Government Printing Office 1970.

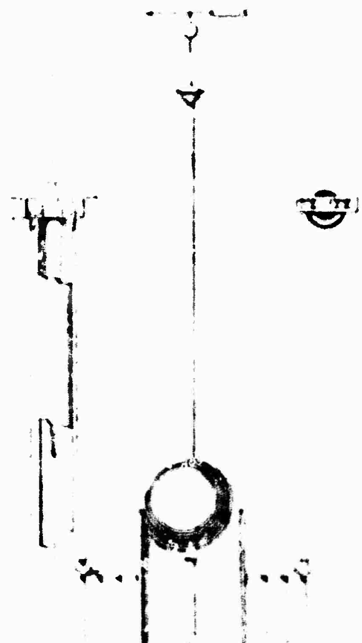


Fig. 20. Time ball design by W. F. Gardner. U. S. Patent No. 334,167. From National Archives, Record Group 37, Entry 64, 1482, 1885.

Fig. 21. Time ball on Western Union Building, NYC. From *South Street Reporter*. XI (Winter 1977-8), 15. Reprinted with permission.

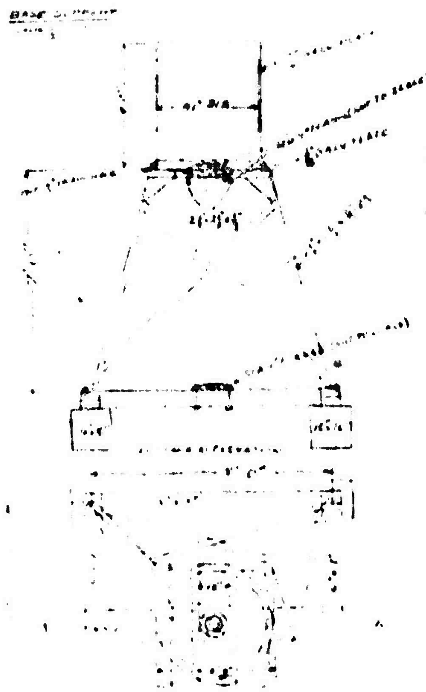
the second time ball structure on the Western Union building in New York City. This next sequence is from the Archives of the Savannah Hydrographic Office. Figure 22 shows some sketches of supports for the retaining tub, and they are obviously from blueprints. The rest of the structure shows in Figure 23, with the time ball at the top. Figure 24 shows the time ball as a series of ribs covered with canvas. Now we see them totally the same. Figures 25, 26 and 27 show Duluth, Norfolk and Philadelphia, respectively.

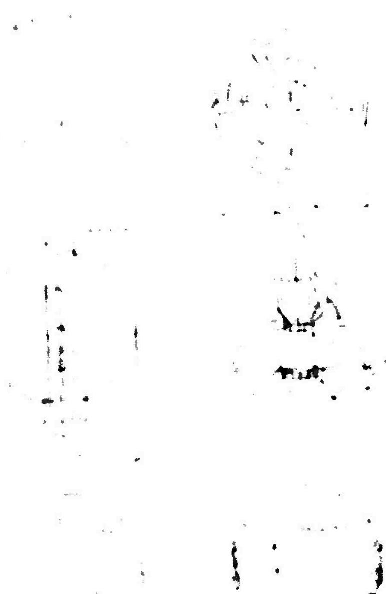
I mentioned that people were very excited about electricity and science and getting accurate time, and they wanted time balls. However, Western Union had a veto on any time ball structure that was in land in the United States. Figure 28 shows one of the last ones, on a then new Baltimore building. We see a ring structure with small time balls and retaining tubs (the real time ball is at the top of the staff). They worked the time ball concept into the ornamentation of the building.

Now, time balls st being dismantled, destroyed and so on. Figure 29 shows the Detroit one. t picture is from the turn of the century, and it looks like they are destroying the whole neighborhood!

Figure 30 is the one that Sharon Gibbs likes the best. It is from the National Archives and is dated 1937. The lady in the picture has a watch on, there is an alarm clock in her hand, and it says "Save the San Francisco Time Ball." And so to save $200 a year, the Hydrographer of the Navy abandons the time ball in San Francisco.

Figure 31 shows Mare Island Naval Shipyard. Three of the buildings (the time ball is on the closest one) were National Historic Landmarks, and I was excited about that. Then I learned, three months ago, that these land marks had been dismantled. There were good reasons for doing so: this is prime property on the base, the ferry boat comes in here, and so on. The argument





Sketches for the Savannah time ball. From National Archives, Record Group 37, 232883-187 Savannah.

Fig. 22 (top left). Details of retaining tub and supports.

Fig. 23 (top right). Total structure, with ball raised.

Fig. 24 (bottom). Details of ball and cone at top of staff.

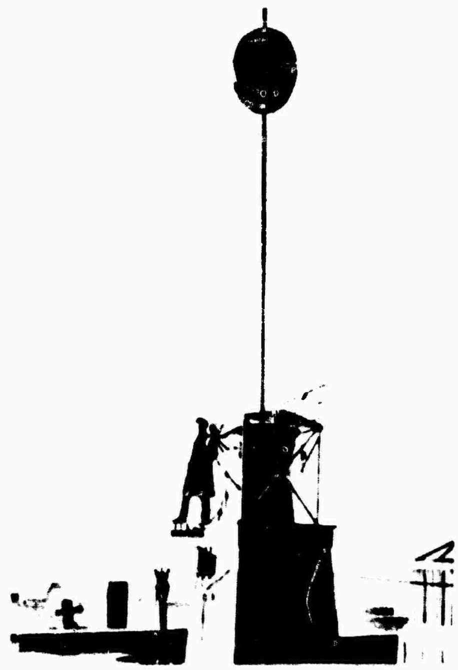


Fig. 25 (top left). Torrey Building, Duluth. From National Archives Record Group 37, 57438; 1911.

Fig. 26 (top right). Citizen's Bank Building, Norfolk. From National Archives Record Group 37, 56846; Nov. 8, 1911.

Fig. 27 (bottom). Philadelphia time ball. From Naval Oceanographic Office, Bay St. Louis.

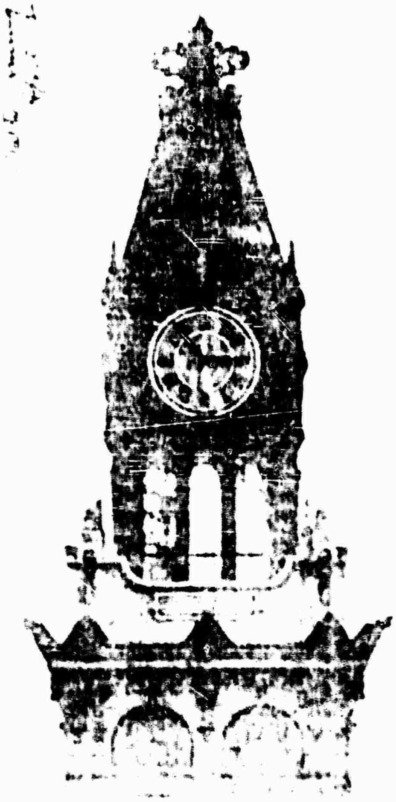


Fig. 28 (left). Maryland Casualty Building, Baltimore. From National Archives, Record Group 37, 151070-187 Ba.: April 24, 1918.

Fig. 29 (bottom). Time ball on Wright, Kay & Co. Building, Detroit. From Detroit Public Library, Burton Historical Collection.

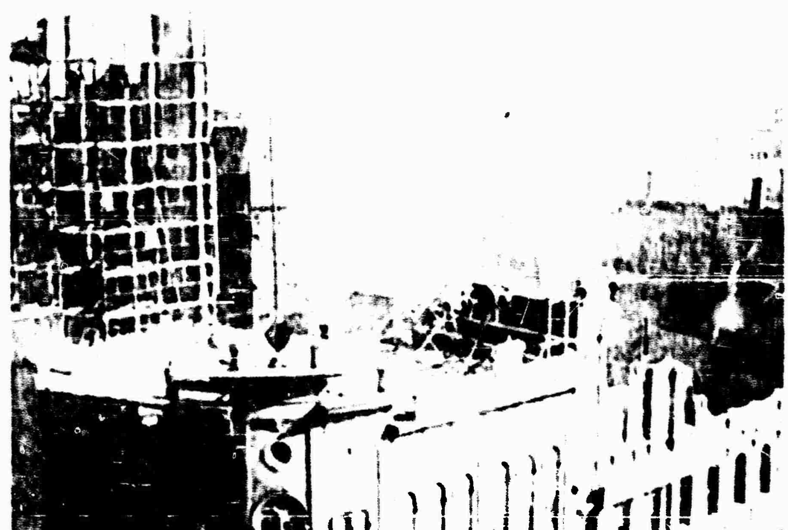


Fig. 30. Fairmont Hotel, San Francisco. From National Archives, Record Group 37, 142716 SF; March 15, 1937.

is that there are other examples of industrial buildings of that era. Those buildings had been constructed about 1865.

The building in Figure 32 is the Prudential Building in Buffalo. The architect was Louis Sullivan. This is considered his finest example of a commercial building, so Buffalo is spending seven million dollars to restore it. I'm afraid they might ask for the time ball structure again.

Seaman's Church Institute in New York City is shown in Figure 33. The structure at the top is the Titanic Memorial Light and Time Ball Tower, shown in close-up in Figure 34, and it was in service from 1913 (the Titanic sank in 1912) until 1967. When the building was demolished, Exxon spent $200,000 to build the Titanic Memorial Park and put the time ball structure (shown as Arabic 1 in Figure 35) as the gateway to the South Street Seaport Museum.

There are a few time balls left. Figure 36 shows the one at Greenwich. And there is the time ball in Times Square at New Year's Eve. I should point out here that this time ball comes from a somewhat different tradition. The time, the noontime we are talking about, is the *moment of release* of a time ball. Here it is a bit different because these people usually are having a lot of trouble navigating at this time. You make the ball float down very slowly. There is a man named Russell Brown, who probably is the only non-scientist interested in the leap second, and it is his job to make sure it lands exactly at the New Year.

We have now seen some time balls. Let me stop here and say, thank you very much.

R. E. Schmidt (Nautical Almanac Office): Do you know anything about a time ball in St. Louis? There was a reference in the publications of the Morrison Observatory.

Bartky: Yes. I read a couple of papers of H. S. Pritchett, who was initially here and then went to Morrison. He was also director of the observatory at

Fig. 31. Mare Island Naval Shipyard, California. From S. Lemmon and E. Wichels, *Sidewheelers to Nuclear Power.* Annapolis: Leeward Publications, 1977. Reprinted with permission.

Fig. 32. Prudential Building, Buffalo. From Louis Sullivan Architecture Museum collection.

Fig. 33. Seaman's Church Institute, NYC. From South Street Seaport Museum collection.

Fig. 34. Titanic Memorial Light and Time Ball Tower, Seaman's Church Institute, NYC. From South Street Seaport Museum collection.

Washington University (St. Louis), then was superintendent of the Coast and Geodetic Survey, and then president of M.I.T. He is very unhappy with the Naval Observatory because the Naval Observatory is interested in Standard Time. He loses business because of that. Morrison Observatory had two time balls, one in Kansas City and one in St. Louis. Rather than talking about those, I wanted the talk to focus on the Naval Observatory and the sea navigation time balls.

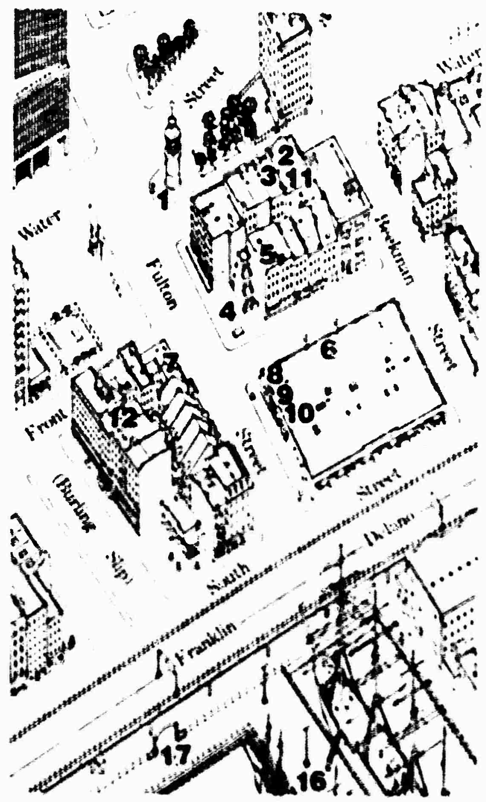


Fig. 35. Titanic Memorial Park, New York City. From South Street Seaport Museum. Reprinted by permission.

Captain R. A. Vohden (Superintendent, USNO): What were they made of? About how much did they weigh?

Bartky: About 120 pounds. They started off being ray (segment) structures and then there was that huge solid shell one in Boston, which was 400 pounds. They ended up as a framework covered with black canvas. Then they were quite reasonable in weight.

R. T. Clarke (Time Service Divison): In one of your examples, you said that the loss was $66,000 to one observatory that was providing time. How did astronomy survive the loss of this much money in 22 places?

Bartky: I have no idea. Maybe this was the time of the large endowments that were starting. I simply don't know. That is a lot of money.

Clarke: Did the other 21 observatories also complain?

Bartky: That was the only one I have seen where there was that amount of money. Pickering at Harvard is very concerned that his observatory gets to drop the time ball in Boston. He says two things: we provide the time from

Cambridge, and we receive not inconsiderable sums of money. There is money here. And it is obvious that Western Union finds out that there is a great deal of money in it too, and that is why they wipe out the others. Now that is a good thing, because I would much rather get one source of uniform time than to have to do some sort of averaging among 23 observatories. So the right thing happens, but we might not like it.

Clarke: How many observatories went out of business that way?

Bartky: The small ones did, but the big ones . . .

T. E. Corbin (Transit Circle Division): What happened after a week or two of cloudy weather?

Bartky: The error got up to a second to two seconds and that is about as bad as it was. They had reasonably good clocks.

Corbin: Back in the 1850s a week or two might get you up to several seconds.

Bartky: I agree; I was talking of 1885. There is a reference in one of your Annual Reports, where in 1880 Gardner takes apart the mean time clock, cleans it and puts it back together. Suddenly, it works better than it ever has before. They say the rate is, at the very worst, a tenth of a second a day, which is very good. He was obviously an exceedingly clever man.

G. H. Kaplan (Nautical Almanac Office): The time ball in New York City. Was that involved in the real time balls?

Bartky: No, I don't think so. The New York Times occupied that building until 1913 so it's easy to search in the literature. It is one newspaper, once a year, so you read about it. What they did is drop it down; as soon as it hit, lights on the three sides of the building were turned on showing the New Year. That was another excitement for the general public using electricity. No, it is not in this tradition, but there is instant recognition for it. When you call historical societies and say, "Time Ball," they don't know what you mean. But when you say, "New Year's Eve," then they do.

S. J. Dick (Transit Circle Divison): Do you care to say a few words about the controversy over the Washington time ball? Why there is a confusion of

dates?

Bartky: I have looked through the history, and we have gone through the Archives. You have notes of dates and I borrowed one set from Dr. Glenn Hall, and then I looked through your Annual Reports, and so on. The notes say 1844-45, and that seems about right because of the picture of the time ball on the title page of the *Washington Observations*.

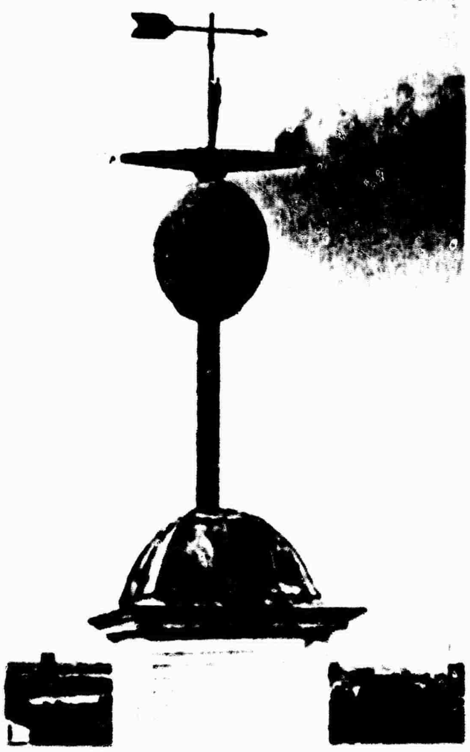


Fig. 36. The Greenwich time ball today. With permission, National Maritime Museum, London.

Dick: The date of the *Washington Observations* is 1846.

Bartky: Yes, two years here I would not mind at all! But then you see Pritchett's paper. He was here; he was born in 1857, but he was here as a young man and obviously he talked to people. He was the one who says, "You drop it on the dome, it rolls down the dome onto the roof," and he is the one who says, "1855." Then there is one other reference, Loomis' *Progress in Astronomy*. The first edition does not talk about it. The third edition I have not seen yet. Have you seen that edition?

Dick: Yes, just today. It has a reference to it in 1856. It does not give any specific date.

Bartky: As you know, that is just an upper bound. There is visual evidence of an 1846 date that is the slide that you saw but there is no written evidence until 1855 to 1856. The Archives are tremendously big, and the question is, do we have an idea how to get into them fast? They talk about 20 feet, 100 feet of records, and so on. How do you find it? And so you think, the astronomers were observing maybe they wrote it down in their data books, and then that might do it faster. There is just a tremendous amount of material to go through in the Archives.

NOTES

1. Steven J. Dick, "How the U. S. Naval Observatory Began, 1830-1865," *Sky and Telescope*, 60 (1980): 466-471. Reprinted with corrections and footnotes in this volume.

2. Since the Symposium, some additions have been made to the chronology. See the paper by I. R. Bartky and S. J. Dick, "The First Time Balls," *Journal for the History of Astronomy*, 12 (1981): 155-164.

3. For evidence found since the Symposium, see the paper by I. R. Bartky and S. J. Dick, "The First North American Time Ball," *Journal for the History of Astronomy*, 13 (1982): 50-54.

4. See footnote 2.

OBSERVERS AND THEORETICIANS: ASTRONOMY AT THE NAVAL OBSERVATORY, 1845–1861

Marc Rothenberg
Smithsonian Institution

This afternoon I would like to provide some context and background for the Naval Observatory and its work during the two decades prior to the American Civil War, specifically during the period of the superintendency of Matthew Fontaine Maury. This was a period of tremendous growth in the American astronomical community. The seven observatories that existed in the United States in 1840 had become thirty by 1861. None of the observatories active in 1840 was considered a center of significant research. Two decades later at least six observatories were able to conduct important research.[1] An American youth in 1861 who wanted to become a professional astronomer could anticipate finding experienced and skilled instructors and employment opportunities.[2] By European standards, Americans were generally not in the forefront of research. However, they were viewed as competent astronomers who occasionally exhibited flashes of brilliance, not yet in the same class with the best of Europe, but as a community probably comparable to the Russians and Italians, and gaining rapidly upon the English, French, and Germans, who constituted the astronomical elite at the time.[3]

There were a number of factors contributing to this great growth. Part of it was the innate interest that astronomy generated among the general public. It is a very fun science, and astronomers were able to exploit this interest to gain financial support. International rivalry also played a part. Achievement in science was viewed as one benchmark in a nation's progress in international competition, much as the Olympics perhaps may be viewed today. To have first class observatories and a first class scientific community was important to some Americans, especially when they looked overseas at European accomplishments. Finally, there was the role of sectional rivalry. One of the factors in the ability of the Harvard faculty to raise large sums of money for Harvard College Observatory was the fact that both Philadelphia and Yale had observatories. How could Harvard (and therefore Boston) not have some sort of observatory on the same scale?[4]

The community that developed in America during these two decades had a number of very clear characteristics. One of the interesting things I have found is that the men who staffed the Naval Observatory during the antebellum period (during these discussions I will exclude the passed midshipman and other temporary naval staff who were stationed here, concentrating on the professional astronomers of the staff) were in many ways very typical of the American astronomical community as a whole, particularly regarding education and experience.[5] Most American astronomers in this period were college educated, and it was during their collegiate days that they were formally introduced to the science of astronomy, although with relatively little hands-on experience. The observatory staff was also made up predominantly of college graduates. More particularly, the Naval Observatory staff had been educated at places like Yale, Harvard, Bowdoin, and Middlebury at a time when most American astronomers had been educated north of the Mason-Dixon Line and east of the Appalachians, with Yale and Harvard being the leading institutions. Like those astronomers outside the Naval Observatory, the staff members of the Naval Observatory who undertook the most sophisticated research were those who had received some sort of specialized training or had acquired a knowledge of the foreign literature, particularly that of the German astronomical community, the acknowledged leaders in practical astronomy of the period. Those astronomers who were self-taught, about twenty percent of the astronomical community during this era, usually offered as partial compensation experience as observers, particularly as surveyors. Moreover, many of the self-taught had early in their careers become attached to the academic community, inculcating its values and norms; i.e., they had become part of the professional scientific community, which had been centered in the academic world. So, too, was it in general with the self-taught astronomers of the Naval Observatory.

In terms of training and past experience, the Naval Observatory staff was atypical of the American astronomical community as a whole in only one regard: the lack of any West Point graduates on the staff. The United States Military Academy ranked third in this period as a source of education for American astronomers. With this exception, the Naval Observatory had a very representative group of American astronomers.

If their educational experience was typical, their administrative setting, their institutional setting, was very atypical. Most American observatories of this period consisted of one-or two man staffs. If there were any junior staff, they were usually there in the role of students, not as skilled astronomers. If there was any hierarchical structure, it was in the nature of a student-teacher hierarchy. Thus, the Naval Observatory was very unique

in that it had a hierarchical scientific structure consisting of master astronomers. There were more astronomers at the Naval Observatory during the 1840s and 1850s than at any other institution in the United States, unless one counts the mass of students that appeared at the Harvard College Observatory toward the end of the 1850s. But those were students, not skilled astronomers. Indeed, the Naval Observatory as an institution was much closer to European analogues than anything in America, with the closest analogue being Greenwich Observatory. This analogue I will explore in more detail in a few minutes.

Now I would like to make another point. Compared to most astronomers active in the United States during this period, the staff of the Naval Observatory has had a very low historical visibility. I would wager that few historians of astronomy or of American science recognize their names, while few astronomers could identify them. One does not usually find their names in the histories of astronomy or even in the folklore of American science. They had generally few articles to their credit, and in the broad scheme of American and world astronomy, most appear, at first glance, relatively insignificant.

The analogue with the contemporary Greenwich Observatory is quite distinct. There, too, one finds low historic visibility. For example, how many 19th-century staff members of Greenwich, aside from the Astronomers Royal, can one usually name? The typical historian would have problems coming up with even one. Do most historians of astronomy have any idea how many staff members there were at Greenwich? I think the answer generally would be no. Again, one finds relatively low productivity measured in terms of published papers, and again, relatively few important accomplishments that made the history books, or even the textbooks of astronomy.

I would argue first that in both cases we have hierarchical structures, the Naval Observatory under Matthew Fontaine Maury and Greenwich under G. B. Airy. Secondly, the research programs were generally (although not completely) limited to the painstaking work of astrometrics rather than what history books look on as the more glamorous astrophysics. And thirdly, in both cases we have the diffusion of resources among a number of fields of science; we are familiar with the fact, for example, that Maury was interested in meteorology and hydrography, but perhaps less familiar with the fact that under Airy Greenwich conducted research in both meteorology and geophysics. To Maury and Airy these national observatories were not merely astronomical observatories, but also centers of research in a broad spectrum of the physical sciences. This tended to limit the accomplishments

of staffers in a particular area because both resources and time were dissipated.[6]

One can illustrate here the restrictive nature of the research at these two observatories by comparing and considering lists compiled by Stephen Brush of the top ten astronomers and astronomical accomplishments for each of three fifty-year periods from 1800 to 1950.[7] I have certain problems myself with some of the specifics of the lists, but one of the interesting things about them is that they are compiled with the benefit of hindsight, i.e., they reflect pretty much 20th-century attitudes of what was important in astronomy in the 19th century. For the 19th century as a whole, ten American and ten British names appear on various lists. Only two of these are on the staff of the Naval Observatory. I suppose you can all guess they are Simon Newcomb and G. W. Hill. No member of the Greenwich Observatory staff appears. Was Greenwich simply filled with non-entities? No, again it was that the nature of the research done at these places does not catch the eye of the historian. Historians look at the forerunners of astrophysics and great discoverers, not at the men who spend their evenings in this very fundamental, very basic work. Academic astronomers and independent amateurs made the very unusual, spectacular, and glamorous discoveries. They had the opportunities and freedom which the restrictive, fundamental research programs of the national observatories did not grant.

What of the men who were at the Naval Observatory during this period? They were, as I have said, fairly typical in education and in career; yet they were very diverse in personality. I would like to give you a few insights into some of these men. Perhaps the best of those who stayed at the Naval Observatory for any extended period during this time was Joseph Stillman Hubbard (1823 1863), who was elected to the National Academy of Sciences as one of the founding members.[8] Hubbard was Yale educated, graduating in 1843. He then went to the Philadelphia High School Observatory, where he was part of an informal (one might say "graduate") education program where he learned the fine points of observational and positional astronomy under Sears Cook Walker. He was appointed to the Naval Observatory in 1845 through the intervention of Alexander Dallas Bache of the Coast Survey and John C. Frémont, the famous explorer. Hubbard had reduced Frémont's observations during his Rocky Mountain expedition. Parenthetically, let me point out that one of the themes one finds through this period is that it's not only how much astronomy you know, it's whom you know that often leads to an appointment at the Naval Observatory. The stronger the lobbying effort behind you, the better chance you have of being appointed. At a time when few qualifications could be formalized or

quantified, personal recommendations were essential.

Hubbard was an all around astronomer, who proved to be equally at home doing observational or computational work. His major contribution was his discussion of the orbit of the great comet of 1843, which was spread over seven issues and two years of the *Astronomical Journal*.[9] He was also, incidentally, Associate Editor of the *Astronomical Journal* and has been credited by the Editor as the man who actually saw it to press most of the time. His work on the great comet was a rigorous treatment of the observations of the comet and a calculation of the orbit. It also proved to be a reference piece for Americans who did not know enough German to follow the discussions of the comet in that language. Hubbard very nicely included appendices in which he listed equations and discussed what the Germans were doing, knowing full well that most Americans could not read the articles being published in the *Astronomische Nachrichten*. Hubbard was, unfortunately, one of those who suffered from the bad physical conditions at the Naval Observatory, and died shortly after his election to the National Academy. It represented a very great loss.

One of Hubbard's contemporaries was Ruel Keith (1826-1908),[10] who graduated from Middlebury College in 1845. Almost immediately there was a letter from Maury promising him a position at the Naval Observatory as soon as one appeared.[11] He became a favorite staff member. Keith was an observer, pure and simple. In fact, when he left the Observatory he earned his living as a surveyor. He was in favor at the Naval Observatory under Maury, I believe in all honesty, because he was, unlike some other staff members, "an attentive officer."[12]

Among the other staff was James Major (1813-1898), born and educated in Ireland, like Keith an observer pure and simple.[13] He became very friendly with his fellow Irishman and astronomer Father James Curley at Georgetown University and eventually left the Navy in 1859 to become a Jesuit.

The senior staff member was James Ferguson (1797-1867)[14] whom I look upon as the perennial bridesmaid. Ferguson was a surveyor, essentially a self taught astronomer, but a very experienced and skilled observer. A member of the Coast Survey from 1833 to 1847, he rose to the position of first assistant. In 1843 he was passed over for the position of superintendent when the Secretary of Treasury, under tremendous pressure from the scientific community, brought in Alexander Dallas Bache from the University of Pennsylvania. Ferguson was very bitter about the fact that he was passed over; this was resolved a few years later when, after much fighting with Bache, he joined the Naval Observatory staff, where he became one of

James Ferguson (1797-1867), Naval Observatory astronomer, 1848-1867.

the most prolific publishers in the American astronomical community. He published over 80 articles in journals, primarily very straight-forward observational pieces on the positions of asteroids and comets.[15] These were not particularly earthshaking contributions, but indications of a man who worked very hard.

When Charles Henry Davis left the Observatory as Superintendent in 1867, Ferguson campaigned to force the Navy to obtain a civilian director for the Naval Observatory, working on the assumption that as the senior civilian on the staff he would probably get the job. His effort to get a civilian director drew very enthusiastic support from the American scientific community, although ultimately it failed; a military director was selected. Even worse, Ferguson found that although people liked the idea of a civilian director, he was not necessarily the one they wanted. Names other than James Ferguson had been circulated.[16]

So we have an institutional structure and interesting people. But there is a third leg to this triad. We also have a director, a superintendent. Ultimate responsibility for the way in which research was conducted at the Observatory during this period has to be laid at the feet of the Superintendent, Matthew Fontaine Maury (1806-1873).[17] He is a man who has generated tremendous controversy among both his contemporaries and historians.[18]

To me, Maury was an outsider who never learned the rules of the game. He was not an astronomer; he was not a professional scientist; he was not a part of the academic community. Almost everyone else in the American astronomical community was a professional scientist, an experienced observer, or a member of the academic community. There was a fairly well established invisible college, but Maury never really became part of it. He was essentially self-taught, knowing much about the content of astronomy but little about the fine points of doing it, particularly in an academic environment. He was the only director of a major observatory in the United States who had neither an academic degree nor continual extended contacts with the academic community.[19] Maury, unfortunately, was isolated.

Maury probably never realized the extent of his own ignorance; or put another way, he never realized how naive he was in terms of his dealings with the American astronomical community. His naiveté is vividly expressed in a letter he wrote Jared Sparks, then President of Harvard, on March 10, 1849, suggesting a cooperative research program between Harvard and the Naval Observatory.[20] Among his first errors (if one was a member of the invisible college, one would recognize this immediately) was addressing the president of the college directly, in effect going over the head of the observatory director. This implied, of course, that the President of Harvard and not

the director of its observatory established the research agenda of the Harvard College Observatory. William C. Bond, the Director, would never recognize the right of the president to establish directly a research agenda. Indeed no professor allowed his research agenda to be established by higher administrative authority (except perhaps indirectly, through the manipulation of funds). Already, Maury was on the wrong foot.

Then there was the proposed program itself: "The plan is to assign magnitude, position, & colour to every star, to obtain accurate drawings of all the nebulae, correct maps of all the clusters, with proper measurements of the binary stars, so that when the undertaking is completed, it shall form a great American work as complete in all respects as instruments & the present state of astronomical science will admit it to be."[21] There is grand vision here, but unfortunately little touch of reality. The labor—and one doubts Maury really realized this—was tremendous, especially the reduction of observations. Bond, the Director of the Harvard College Observatory, declined on the grounds that his staff was insufficient to become involved in such a project.[22] (As it turned out, in reality neither was the staff of the Naval Observatory.)[23] Indeed, the alternative which Bond selected was to undertake particular aspects of Maury's research program, such as stellar positions and nebulae, but in very small, discreet, do-able chunks. Bond did not propose to produce a grand catalogue, but detailed observations of certain select nebulae and stellar "zones," at the same time remaining flexible to investigate other research problems, such as comets and observations of Saturn.[24] Maury represented a man who simply had no concept of the time involved in doing research in astronomy.

Finally, Maury suggested that Harvard College Observatory should publish its results under the auspices of the Naval Observatory, not a good political move at all. Bond declined, very politely, pointing out that Harvard College Observatory published with the cooperation of the American Academy of Arts and Sciences. Indeed, if Maury knew anything about Harvard College Observatory he would have known that most of the financial support for the observatory had come from the American Academy and its members, that there were very intimate bonds between Bond and the Academy, and that it wasn't accidental that most of the observatory's work was published in their proceedings.[25] Moreover, didn't Maury recognize that Harvard might want to publish its own annals, which indeed it did,[26] as did every major observatory when it had sufficient funding? Maury was a man who seemed out of synchronization with the rest of the astronomical community.

Just to compound the problem, Maury was accused of misusing his staff.

Again, here I would argue it was more through ignorance than intent. The most glaring and most famous incident was his relationship with Sears Cook Walker (1805-1853), a man whose name should be better known to historians of astronomy and to astronomers.[27] He was the leading theoretician on the Naval Observatory staff prior to Simon Newcomb, although he was only there for a short time. Walker was a Harvard graduate who had come to realize the power of the astronomical methodology developed in Germany by Gauss, Encke and Bessel. From the mid-1830's on, he became an ardent student of astronomy while working as an actuary for a Philadelphia insurance company. He became the director of the Philadelphia High School Observatory upon its opening in 1839. Despite the fact that it was at a high school, this was the only research observatory in the United States in the early 1840s.

At the time of his appointment to the Naval Observatory staff in 1846, Walker was probably the leading astronomer in the United States, or at least in the top three. Maury did not have an inkling of this. Writing to Benjamin Peirce of Harvard, who was one of Walker's chief competitors for the title of number one, Maury stated that "I can boast assistants here in no whit his [Walker's] inferiors."[28] As an administrator, I can compliment Maury on his loyalty to his staff, but when a director claimed to have assistants equal to the best man in the field, when knowledgable astronomers knew that the claim was invalid, the credibility of the director became questionable. The truth was that Maury had only one assistant even in the same league with Walker, and that was one of Walker's former students, Hubbard. Maury's attitude toward Walker was quite straightforward; Maury was the Superintendent, and Walker would do astronomy as he was told to do it. In a long letter sent to Walker prior to his taking up of duties in which Maury detailed the new staff member's responsibilities, Walker was instructed in some detail how to observe, as if he were a novice.[29]

What is probably most surprising is that Walker lasted as long as he did—one year. He obviously did need a job. Finally, unable to accept the misuse of his talents,[30] Walker joined the Coast Survey under Alexander Dallas Bache, becoming head of the Longitude Division. He later went insane and died in an asylum.

When one measures the accomplishments of the Naval Observatory during the period 1845-61, one has, I believe, two major considerations. One is the potential, the other is the reality. Potentially, the very structure of the Observatory as a hierarchical institution with a particular research program placed certain limitations on its flashy accomplishments, if I may use that term. On the other hand, it had tremendous potential to do excel

lent astrometric work. Generally, it fulfilled that potential quite well. The great sin that Maury committed in the eyes of the astronomical community was his de-emphasis of the reduction of observations. Because he never really realized the finer points of astronomical research, he never understood the principle that you had to spend some time reducing your observations. Unreduced observations are not of much use. The backlog of observations was tremendous.[31] In the wake of Maury's departure to the South, Congress appropriated funds to reduce the backlog of observations that had accumulated at the Naval Observatory. The observations were turned over to B. A. Gould, an astronomer at the Coast Survey, who promptly reduced them over the course of a year.[32]

When one looks at the Naval Observatory during this period, one has to say it was a fine place, which unfortunately, because of a director who was outside the academic and astronomical networks, never quite accomplished all it could have. Thank you.

S. J. Dick (Transit Circle Division): You can tell these papers are uncensored. Are there questions for Marc?

Q: Were there any complications from these passed midshipmen and other people who were dumped on the observatory as a nice place to keep them, rather than have them starve as far as the Navy was concerned?

Rothenberg: From the point of view of the community as a whole no one seems to say much. I think they are perceived as people who are doing the very, very routine sort of work which everyone was very happy to let someone else do. In an academic institution you might bring in a senior student to do the same sort of thing. A student who expressed interest in astronomy could nurture such a position over the four years. I think they were viewed somewhat in that sense. I don't know of any of them who entered the Observatory in that capacity who ever went on to careers in astronomy per se. Some went on to careers in geodetic work for the Coast Survey, for example, but I'm not sure any of them ever entered astronomical careers. I don't know if they were discouraged by their experience, or what.

Dick: But Gilliss began as a passed midshipman.

Rothenberg: Yes, that is prior to the period I've discussed today.

A. L. Norberg (National Science Foundation): Marc, there are a number of

interesting directions we could follow in your talk. One of them has to do with the biases of historians. We commonly look at the writings of the scientist, and in the second half of the period that you are talking about today, I looked to see what the Navy's attitude was, not the Superintendent, but people above the Superintendent all the way up to the Secretary of the Navy. I did not find anything. Did you find anything for your period about what the Navy hoped to achieve?

Rothenberg: No, I'm not familiar with this. I have not conducted extensive work. The closest analogy that I know of would be the Coast Survey with the Secretary of the Treasury, where I know that for the most part the Secretary of Treasury was very happy that the scientific community was taking some of the responsibilities of selecting personnel off his hands. I don't know, for example, what the Secretary of the Navy felt about this sort of thing. One of the interesting questions is what does the Secretary of the Navy feel when he is getting all sorts of letters asking that certain people be appointed to positions, and when he has no technical expertise to figure out and evaluate these recommendations. That's a good question I haven't really researched.

P. K. Seidelmann (Nautical Almanac Office): Have you looked at why the Nautical Almanac Office was set up as a separate entity at one time, and whether that has some connection with the reputation of the Naval Observatory?

Rothenberg: That is the direct result, I believe, of Maury's reputation. It was placed in Cambridge, next to the best research observatory in the United States, near Benjamin Peirce, a leading mathematical astronomer by the mid-1850s, and as far away as possible from the mismanagement of Maury. Peirce was de facto scientific director of the Almanac, while both the first two directors Charles Henry Davis and Joseph Winlock were his protégés. I think this is the way the astronomical community wanted it to be. They simply wouldn't trust Maury with it. They knew men like Bond and Peirce and Davis. Everyone thought very highly of Davis, and one of the key things was to make sure that Davis could do things his way, because he was held in tremendous respect by the astronomical community. Basically he was one of their own. He was part of the invisible college.

A. N. Adams (former Director, Six Inch Transit Circle Division): You haven't mentioned Gilliss, who had a lot to do with the early astronomical observa-

tions in the country, and in founding the Observatory. It's funny that somehow he didn't get to be the first Superintendent, and Maury did. Do you know anything about that?

Rothenberg: Gilliss was at the Observatory prior to Maury and comes back as Superintendent after Maury. I think the Chilean expedition of Gilliss is a fascinating astronomical expedition. Steve Dick touches a bit on this in his article. There is an aspect here again of politics. Maury is a Southerner. There is a tremendous Southern lobby in the United States government during these periods. Maury gets at least one promotion through the Southern lobby when he is under attack by the generally Northeastern scientific community. In a gesture of defiance, pressures are placed on the Secretary of the Navy to make sure that he gets his promotion. This political aspect overlies a lot of things in this period which I have not gone into in detail. But this is an aspect.

NOTES

1. Philadelphia High School Observatory, Harvard College Observatory, the Naval Observatory, Cincinnati Observatory, Detroit Observatory (University of Michigan), and Dudley Observatory.

2. T. H. Safford, "The Development of Astronomy in the United States," *The Sidereal Messenger*, 8 (1889), 199.

3. Quantitative evidence to support this conclusion is difficult to find. One crude measure of the comparative strengths of the different astronomical communities is the evaluation of the Royal Astronomical Society. In the 1859 list of Associate Members, the United States ranked fourth, behind Germany, France, and Italy, having six of the fifty Associate Members. The free distribution list for the *Monthly Notices* contained sixteen Americans out of a total of one hundred and twelve individuals. This placed the United States third, behind Germany and France. *Monthly Notices of the Royal Astronomical Society*, 19 (July 8, 1859), 328-335.

Still another way is to count observatories. Unfortunately, methods for dating the founding of observatories differ from historian to historian, resulting in counts which do not agree. However, counting only observatories which remained active through 1931, a method which is prejudiced against the ephemeral private observatories so prevalent in the United States, one historian found that the United States ranked second only to Germany in the number of active observatories in 1860. D. B. Herrmann, "Zur Statistik von Sternwartengründungen im 19. Jahrhundert," *Die Sterne*, 49 (1973), 48-52.

4. Bessie Zaban Jones and Lyle Gifford Boyd, *The Harvard College Observatory: The First Four Directorships, 1839-1919* (Cambridge, 1971), pp. 37, 50.

5. These generalizations are based upon my detailed study of the American astronomical community which was summarized in "The Educational and Intellectual Background of American Astronomers, 1825-1875" (Ph. D. dissertation, Bryn Mawr College, 1974).

6. For Greenwich Observatory during this period, see A. J. Meadows, "Airy and After," *Nature*, 255 (June 19, 1975), 592-595.

7. "The Rise of Astronomy in America," *American Studies*, 20, No. 2 (1979), 41-67.

8. B. A. Gould, "Memoir of Joseph Stillman Hubbard, 1823-1863," *Biographical Memoirs of the National Academy of Sciences*, 1 (1877), 1-34.

9. Joseph S. Hubbard, "On the Orbit of the Great Comet of 1843," *The Astronomical Journal*, 1 (1849-1851), 10-13, 24-29, 57-60, 153-154; 2 (1852), 46-48, 57, 153-156.

10. Edgar J. Wiley (compiler), *Catalogue of the Officers and Students of Middlebury College in Middlebury, Vermont* . . . (Middlebury, 1928), p. 166.

11. Maury to Alexander C. Twining, June 24, 1845, Letter Book I, p. 409, Letters Sent, Naval Observatory Records, Record Group 78, National Archives (hereafter, Letters Sent, N.O.).

12. Maury to Elias Loomis, July 30, 1847, Letter Book II, p. 307, Letters Sent, N.O.

13. Rothenberg, "Educational and Intellectual Background of American Astronomers," p. 155.

14. Nathan Reingold et al., editors, *The Papers of Joseph Henry* (Washington, D. C., 1972-), 2:15-16.

15. These publications appeared either in the *Astronomische Nachrichten* or *The Astronomical Journal*.

16. E.g., Ferguson to Joseph Henry, April 30, 1867, Official Incoming Correspondence, Smithsonian Institution Archives; Joseph Henry to Gideon Welles, May 1, 1867, Official Outgoing Correspondence, Smithsonian Institution Archives; Joseph Henry to Ferguson, May 1, 1867, Private Letterpress, Henry Papers, Smithsonian Institution Archives.

17. The secondary literature on Maury is extensive. A sampling includes: Frances Leigh Williams, *Matthew Fontaine Maury: Scientist of the Sea* (New Brunswick, 1963); Jaquelin Ambler Caskie, *Life and Letters of Matthew Fontaine Maury* (Richmond, 1928); Charles Lee Lewis, *Matthew Fontaine Maury: The Pathfinder of the Seas* (Annapolis, 1927); and Patricia Jahns, *Matthew Fontaine Maury & Joseph Henry: Scientists of the Civil War* (New York, 1961).

18. Contemporary opinion of Maury's major publication is summarized in John Leighly, editor, *The Physical Geography of the Sea and Its Meteorology*, by Matthew Fontaine Maury (Cambridge, 1963; republication of the 8th edition [New York, 1861]), pp. xvi-xxix. Extremely negative evaluations of Maury can be found in the correspondence of his successor, James Melville Gilliss—e.g., Gilliss's letters to Edward Sabine, June 20, [1861], Sabine Papers, Royal Society of London; and his letter to George P. Marsh, July 19, 1861, Marsh Papers, Bailey Library, University of Vermont. The Joseph Henry–Alexander Dallas Bache correspondence is another rich vein of very uncomplimentary descriptions of Maury. One example is Henry's letter to Bache of November 30, 1850, Bache Papers, Smithsonian Institution Archives.

The generally laudatory biographies by Williams and Lewis, cited in footnote 17, represent one perspective on Maury. In contrast, Leighly's introduction to *The Physical Geography of the Sea* points out the scientific weakness of the book. A very unsympathetic view of Maury is contained in Nathan Reingold, editor, *Science in Nineteenth Century America: A Documentary History* (New York, 1964), pp. 145-146.

19. An example of the latter type of relationship with academia was William C. Bond (1789-1859), director of Harvard College Observatory from 1839 until his death. Although an instrument maker and self-taught astronomer, Bond had been in contact with the scientific faculty of Harvard since 1811. Jones and Boyd, *Harvard College Observatory*, pp. 28-30, 40-110.

20. Observatory Correspondence, Harvard University Archives. The Maury-Sparks letter is quoted by permission of the Harvard University Archives.

21. Ibid.

22. William C. Bond to Maury, Retained Copy, March 20, 1849, Observatory Correspondence, Harvard University Archives.

23. For a brief, but very useful account of this period in the Naval Observatory's history, see Steven J. Dick, "How the U. S. Naval Observatory Began, 1830-1865," *Sky and Telescope*, 60 (1980), 466-471.

24. Bond's bibliography appears in Edward S. Holden, *Memorials of William Cranch Bond . . . and of his Son George Phillips Bond* . . . (New York, 1897), pp. 277-283.

25. Nine of Bond's articles appeared in the first volume (1848) of the *Proceedings of the American Academy of Arts and Sciences*. In addition, his son and assistant, George P. Bond, published two articles in the third volume (1848) and one in the fourth volume (1849) of the new series of the *Memoirs of the American Academy of Arts and Sciences*.

26. The first volume of the *Annals of the Astronomical Observatory of Harvard College* appeared in two parts in 1855 and 1856.

27. Rothenberg, "Educational and Intellectual Background of American Astronomers," pp. 81-86.
28. January 26, 1846, Letter Book II, pp. 76-77, Letters Sent, N.O.
29. February 28, 1846, Letter Book II, pp. 89-91, Letters Sent, N.O.
30. The dissatisfaction was mutual. Maury attacked Walker for wanting to select his own research topics and concentrating on computation rather than observation. Maury to Elias Loomis, April 20, 1847, Letter Book II, pp. 243-244, Letters Sent, N.O.
31. Mordecai Yarnall recorded his complaint about observations "lying useless" in a letter to Charles Henry Davis, December 16, 1859, Letters Received, Nautical Almanac Records, Record Group 78, National Archives.
32. The progress of the reduction effort can be traced in the Gilliss-Gould correspondence from the fall of 1861 to the fall of 1862 in Letter Book XIX, Letters Sent, N.O.

Matthew Fontaine Maury (1806-1873), first Superintendent of the Naval Observatory, 1844-1861.

MATTHEW FONTAINE MAURY & FRIENDS?*

Andreas B. Rechnitzer
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It is with great pleasure that I accepted this opportunity to relate to you, Ladies and Gentlemen, some of the highlights in the life of the Father of Oceanography and the relationships that appear to have existed between Maury and his peers.

Matthew Fontaine Maury was born in the Spotsylvania Wilderness on a cold January 14, 1806. At the age of 19 he was aboard the navy vessel *Brandywine* as a fresh-caught midshipman to hear the trumpet call and the command "all hands weigh anchor, ahoy." The *Brandywine* set course for a one year cruise to the Mediterranean. Maury was thus off into a new life.

During this cruise a fellow officer reported "that without assistance, he went through a course of study commencing with the rudiments of Euclid and extending to the higher mathematics of Laplace." Maury also started work on a set of lunar tables at this time, an effort he relished.

Returning to New York in 1830 from a cruise aboard the *Vincennes* was a happy time for Maury. He had been away for four years, and had been around the world, probably learning more about it and his fellow man than any college could have taught him in the same period. He had also learned, in widely differing climates, the role that weather plays in the life of man and the supreme importance to the mariner of the winds and currents of the sea. He had also gained a knowledge of practical seamanship and had studied as much as his limited free time and the very scanty supply of books on board permitted.

On October 6, 1830, Maury reported to the school maintained for the instruction of midshipman at the New York Navy Yard. This school, however, was not well organized, disciplined, or staffed, as most senior officers in the United States Navy in 1830 placed a low value on formal naval education.

At the school Maury set himself to study in earnest for the examination for promotion. The subjects on which he would be examined were Bowditch's *Navigator*, Playfair's *Euclid* (Books 1, 2, 3, 4 and 6), McClure's *Spherics*, Bourdon's *Algebra*, mental and moral philosophy, Spanish or French (he chose Spanish), and seamanship.

He was dissatisfied with the teaching at the New York Navy Yard School and decided he could do better by studying on his own.

On December 22, at his request he was detached from the school and granted an unlimited leave.

Maury daily put in long hours of concentrated work on the course of study he had begun aboard ship and, at this time, progressed to advanced study in the works of the French astronomer and mathematician Laplace. Scientific rules and reasons increasingly occupied his mind.

His whole time, however, was not devoted to books, for pretty Ann Hull Herndon arrived to visit in Georgetown. Maury was a young man who had little trouble deciding what he liked and, once he was clear on that point, he poured out his devotion in unstinted fashion. This was his pattern in work, and it proved to be so in romance. In those winter days of 1831 Maury went to call on Ann every night but always left before 10 p.m., when a watchman blew a horn as a signal for lights to be put out in Georgetown.

Maury was ordered to appear on March 3, 1831, before the examination board then in session in Washington. The examiners were officers who had gone to sea before Fulton launched the steamboat, which they had, at the time, probably joined in calling his folly. They were oldline captains concerned with the practical navigation of a sailing vessel.

When Maury "was questioned as to the lunar problem", instead of repeating the Bowditch formulas, after immemorial custom, he had the audacity to step to the blackboard and work out the question as a problem in spherical trigonometry. The professor of mathematics conducting the examination got lost in trying to follow him, and brazenly declared the demonstration wrong. The midshipman insisted and the officers of the examining board, knowing still less than the schoolmaster, decided after an embarrassed consultation to support the latter on general principles. Accordingly they looked as wise as possible, informed the midshipman that he was all wrong and bade him go to sea again and learn his business. Under this decision Matthew Fontaine Maury lost two years in promotion.

Maury's foolhardy display of knowledge may have satisfied his youthful pride, but it placed him low on the list and was to make his next promotion two years later than was necessary. Maury passed the examination but was rated 27th in a class of 30 when he could have been at the top. The mid

shipman whom he had drilled passed with a high rating. Perhaps his low placement on the list taught him the folly of showing off his knowledge, for there is no record of any later similar episode in his naval career.

Once Maury had passed the examination he was free to pursue his own plans until he received orders to report for active duty.

His next duty assignment was to the *Falmouth*. On the *Falmouth* he had a tiny stateroom to himself, just slightly larger than his bunk, but it gave him a place to write. In this little cubbyhole he sorted his notes and wrote his first scientific paper, "On the Navigation of Cape Horn."

When Maury finished this article, written in any snatches of free time he had on the *Falmouth*, he mailed it to the outstanding scientific publication of the United States at that time, the *American Journal of Science and Arts*, where it was published in 1834.

Maury wrote to his brother at this time and said "I should leave Uncle Sam and his Navy if I had any other means of making my way decently through the world. My pay now is nearly $700 and in two or three years more will be $960. At a moderate calculation, I will not be a captain, things continuing the same, for 25 years."

On May 27, 1834, Maury received a leave of absence. He had been at sea for 9 years, and far from home. In July he married Ann Herndon at Laurel Hill, about 12 miles southwest of Fredericksburg.

On April 30, 1835, he completed his textbook on navigation. A full year later it was published.

Because of his long standing concern about funds and promotion Maury confided in a letter to his brother that he planned to use the publication as justification for deep selection and promotion well ahead of his peers if his proposal was accepted he also anticipated a substantial amount of back-pay. In the hope of carrying out his promotion plan, Maury took his case to the Secretary of the Navy and the President, Andrew Jackson. He saw the President personally. He concurred in the proposal, but the Secretary of the Navy turned him down, despite the fact his book largely replaced Bowditch's work as the Navy standard.

After eight years of discussion, Congress authorized the Navy to send an expedition to explore and survey "The South Sea." Captain Thomas Catesby Jones was selected to command. Maury was chosen by Jones to serve as acting astronomer to the expedition. Happiness was short-lived as this post thrust him right into the middle of a conflict between Commodore Jones and Lt. Charles Wilkes. Wilkes, an officer of marked scientific ability, had been sent to Europe to buy chronometers and other scientific instruments for the expedition. Wilkes showed his animosity for Jones, not Maury, by

passive resistance to Jones' request that the equipment be passed to Maury. The wrangle over the instruments was just the tip of the iceberg of troubles that would beset the start of the expedition. The experience undoubtedly prepared Maury for handling personal vendettas and expressions of jealousy among professionals wherein he would be directly involved.

On his way to report to the *Consort* Maury was involved in a stagecoach overturn in Ohio that flipped him through the air from his perch aboard the overloaded coach. A small town practitioner correctly diagnosed the injury, but botched up the repair. This left Maury crippled and no longer able to perform as a sea-going officer. The dry-docked Maury had spent 15 years in the Navy and did not wish to be cashiered out. Fortuitously in February 1842 he was alerted that the Bureau of Hydrography was to be established and that it could be headed by a civilian or officer. When Maury learned that a civilian was ear-marked for the post he could not tolerate the idea. He pulled strings (they said wires in those days) and reported for duty as the Superintendent of the Depot of Charts and Instruments. This meant that he now was in a position to satisfy a life-long dream to pursue professional and scientific opportunities. At last the long waiting was over and the curtain was about to go up on the main drama of Matthew Fontaine Maury's life.

The new man on the block was soon to encounter two additional newcomers to Washington. Alexander Dallas Bache, the great grandson of Benjamin Franklin, and Professor Joseph Henry were to become his nemeses. Bache would lead the Coast Survey and Henry would become the Secretary of the Smithsonian Institution. It wasn't enough that the three would become competitors for jurisdiction over applied science functions being advanced by the bureaucracy, but the holier-than-thou attitude of college professors at the time attempted by innuendo to place Maury in the category of "a charlatan that should be denied recognition." Let's see how this affected Maury.

Bache had earlier written high praise about Maury's scientific abilities and his textbook on navigation. When Bache reached Washington in 1843 Maury extended every professional courtesy and the two men were seemingly friendly. However, Bache had wanted Lt. Gilliss to be made Superintendent of the Naval Observatory; Bache and friends never changed their opinion that Maury should not have been given the appointment. Bache was also annoyed that Maury was studying, speaking and writing about the Gulf Stream. The great-grandson of Franklin regarded this as a subject that belonged to his family, almost by hereditary right, because of "Honest Ben's" original investigation of the current.

A dominating characteristic of Bache was his desire to win men to a

personal allegiance to himself. Bache soon realized that Maury was not a man who would subject himself to his personal magnetism nor have time for gathering weekly with other men to wine, dine and talk.

Bache's ambitions, jealousies, and especially his fear that Maury's rising fame as a hydrographer might yet cause the Coast Survey to be placed under the Navy are understandable grounds for his enmity of the Superintendent of the Naval Observatory and Hydrographical Office. It is difficult to see how a man of Bache's intellect could have justified the inconsistency of his thinking in insisting that the only worthy scientists were those dedicated in utter purity to "principia" and "theoria", when virtually his whole work from 1843 until his death was in applied or practical science—charting the coasts, supervising weight and measures, and serving on the lighthouse board. Similarly, Joseph Henry, once he took the Secretaryship of the Smithsonian Institution, did much to encourage others to pursue pure research while he personally worked on practical research projects.

Recognition of Maury's contributions came mostly from appreciative Europeans, who were undoubtedly shielded by an ocean from the attitudes and prejudices described above. In contrast, he was so honored by most of the learned societies of Europe that he soon carried a chest full of medals. This is not to imply that he was ignored by the United States. Maury was awarded a recognition from the academic world that gave him a sense of support—an honorary LLD from Columbian College (now George Washington University). This coupled with the LLD he had received from the University of North Carolina strengthened his hand.

The message is clear—whenever a man accomplishes work so obviously meritorious that he is acclaimed by both the general public and his professional colleagues, there are always some associates who for their own reasons are critical. In Maury's case, they also included old sea dogs, his fellow officers, who disapproved of having scientific methods introduced to the Navy. Some officers resented Maury's being kept on duty at the Naval Observatory and Hydrographical Office while they had to go to sea and leave their families for long stretches of time. A handful of officers disliked him personally and begrudged his fame. A trenchant example of their application of spite is reflected in an official communication to Maury on September 19, 1855. A letter from the Secretary of the Navy James C. Dobbin, stating:

> The Board of Naval Officers assembled under the act to promote the efficiency of the Navy, having reported you as one of the officers who in their judgment should be placed on the reserved list and the findings of the board having been approved by the President, it be

> comes my duty to inform you that from this date you are removed from the active service list.
>
> You are, however, not detached from the Naval Observatory. I avail myself of the authority of the law to direct that you continue on your present duty.

If the development had not been so serious, Maury could have appreciated the irony. The reorganization for which he had personally worked had eliminated him, one of its mentors. He could hardly believe it.

Next day Maury wrote Secretary Dobbin. Secretary Dobbin replied that the late Navy Board had reported no reasons for the decision on Maury and therefore the department could not grant Maury's request to be informed as to what accusations were made or who made them. Further, Maury learned that there was no records kept of their screening deliberations.

The arbitrary and capricious action of the 1855 Navy Retiring Board resulted in a flood of officer appeals. Two and one-third years later Maury was vindicated and appointed Commander, as of 15 September 1855. The backpay was more welcome than the belated promotion but hardly worth the agony. The battle had taken a heavy toll out of Maury's spirit and creative energy. It had been a shock to him to learn the lengths to which personal dislike or personal ambition could drive fellow officers. Even Senator Mallory, author of the bill to establish the Navy Retiring Board, turned from being a staunch advocate of Maury to an enemy. Two other senators who had sat on the board also were men not apt to change their attitudes for having lost a battle.

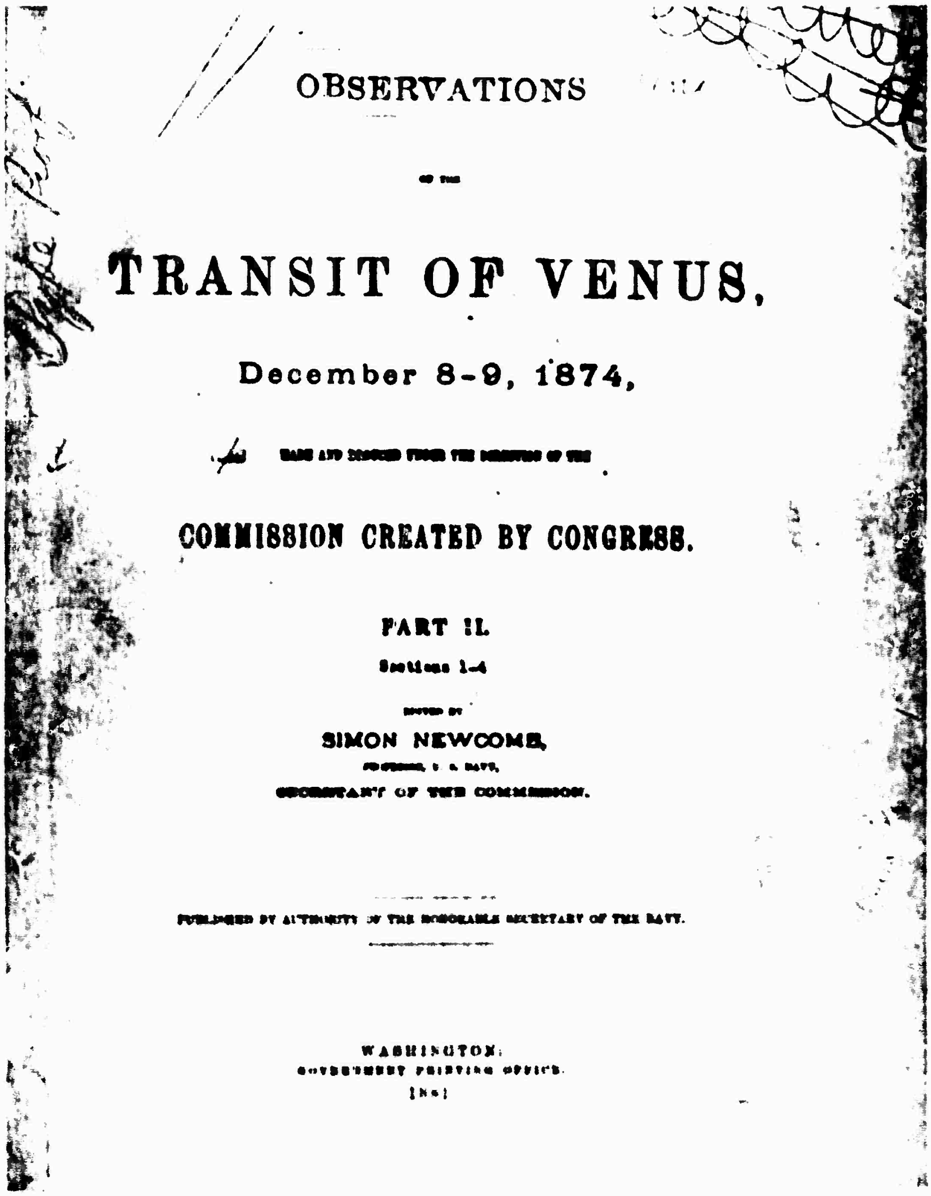
When Maury had first taken charge of the Depot of Charts and Instruments in 1842, he had started his meteorological work on a small scale. When consulted in 1851 by the Secretary of the Navy concerning Britain's proposal for a joint action on land meteorology, Maury had recommended that an international conference should be held to consider a universal system of observations for both land and sea. When Joseph Henry learned of this from Maury he purportedly became alarmed. Henry's cohort Bache appeared in the act to propose that there be a division of functional responsibilities in meteorology that would limit Maury's area of influence. The areas were to be as follows: (1) the Army takes the west, (2) the Smithsonian the east, (3) the British Government the north, (4) the Observatory the sea, (5) the returns of all given to the Smithsonian.

Whatever hopes or illusions Maury may have harbored concerning Joseph Henry's and likely Bache's attitude toward him were at an end when Henry publicly announced that he considered Maury neither a scientist, nor capable

of devising a meteorological plan for the land. Henry and Bache even stated that there should be established a "high tribunal" of men dedicated to pure science to sit in judgement on their fellow scientists, or "pretenders" to knowledge as Bache considered most of the American scientists such as Maury who were outside the coterie of which he and Henry were leaders.

The Civil War, in a way, brought relief to Maury from his Washington tormentors. He returned to Virginia and the Confederate cause.

Maury wrote his own resignation and dispatched it to President Lincoln. Then dressed in a black broadcloth civilian suit Maury walked out of the United States Naval Observatory and Hydrographical Office, on 20 April 1861. His heart was bursting with emotion and he made no effort to hide the tears that flowed down his cheeks. On the Virginia banks of the Potomac not far from where he had gone aboard the *Brandywine* in 1825, Maury turned for one long last look at Washington. What was past was no prologue for him–it had been life itself–work attempted, work achieved–the power and the glory–the hate and the spleen–hurt and disappointments–but always the opportunity to press on to further knowledge.



OBSERVATIONS

TRANSIT OF VENUS,

December 8-9, 1874,

COMMISSION CREATED BY CONGRESS.

PART II.

Sections 1-4

SIMON NEWCOMB,

SECRETARY OF THE COMMISSION.

WASHINGTON:

GOVERNMENT PRINTING OFFICE.

A single set of proof pages is all that remains of the official effort to publish experiences and data acquired by the eight observing parties forming the United States expedition to observe the transit of Venus in 1874.

REMARKS ON THE TRANSIT OF VENUS EXPEDITION OF 1874[1]

P. M. Janiczek
U. S. Naval Observatory

Let us at once dispel any notion that I am qualified to speak to you as an historian. I am not. It happened, however, that my attention was drawn a few years ago to a long neglected document, and my curiosity as to the circumstances of its creation and subsequent fate led me deeper into our subject. The document itself was a carefully detailed report, the only copy known to exist, part of a larger work,[2] for which the scientific community waited in vain until the work had lost all impact and importance.

To appreciate the significance of the subject, to put it into perspective, it is necessary to consider some of the history of the Venus transits, attempts to observe them, and the ends to be attained.

In all history humans recorded only six transits of Venus across the face of the Sun. What follows may give some credence to the idea that whenever astronomers reach a fever pitch of anxiety in attempting to extend the accuracy or quantitative knowledge of some fundamental constant, Nature and man-made circumstances appear to combine to frustrate the effort in direct proportion to the scientific energy expended. Part of this may be understood by imagining, with the aid of recent pictures from space, that one has a vantage point from which the Earth can be watched. Two things would be noted at once: ours is a beautiful blue planet, and it is cloudy most of the time. Astronomers have known this for a long time. They have often tried to increase the chances for success by seeking out numerous and remote places noted for clear skies. Nevertheless, infrequent and transient astronomical events continue to elude observation on account of weather. The frustrations of human manufacture will be evident as we proceed.

The first transit of Venus that could be observed with a telescope was that of 1631. It was night in Europe. By 1639 when the second transit of the eight-year pair was to occur, there was at least an issue to be decided. There were a set of planetary tables by Philip Lansberg which predicted a transit for December 3, 1639. In contrast, Johannes Kepler had declared that after the transit of 1631, there would be no other until 1761. The works of both men were known to Jeremiah Horrox, a young Englishman.

He determined that, although Kepler's predictions were in general more accurate than Lansberg's, the possibility of a transit was worth serious consideration and modest investment of time. Horrox tried to refine Lansberg's predictions, making his own observations and adjusting the tables accordingly, a process not unlike that used today, but without benefit of the calculus or a viable theory of errors. Horrox could not, however, disentangle the actual precepts of Lansberg's tables from the all-pervasive "pretensions to universal accuracy" and self praise which "overloaded" the work. He abandoned that process, deciding in favor of several days observations bracketing both the time predicted for the transit (Dec. 3, Lansberg) and the close conjunction (Dec. 4, Kepler). Horrox watched the Sun throughout the third, but no transit occurred.

December 4 arrived, as did the transit. Horrox was at church when most of it was visible. He was called by some later writers the father of English astronomy; but being an astronomer, he nevertheless had to earn a living by other means, much the same as the general situation today. Horrox was, therefore, a curate and bound by obligations of that office. He hurried to his lodgings afterward, arriving at about 3:15, and finding Venus upon the Sun. He observed what he could of the transit, but in his locale, in that part of the year, the Sun sets before four o'clock. Since he could not see third and fourth contacts, he could not obtain a measurement having any quantitative statement. But he had done much. He had tried more, by calling the coming event to the attention of a number of people. As far as we know, however, he and perhaps one of those with whom he communicated were the only witnesses; and Horrox had almost missed it by being elsewhere.[5]

Around 1700, transits of Venus began to take on added importance. It was suggested that, if observed at points near the limits of visibility on the Earth, they might yield an accurate value for a fundamental quantity of astronomy, the parallax of the Sun, or the mean Earth Sun distance. Considerable enthusiasm greeted the next transit pair which occurred in 1761 and 1769. In 1761, the French astronomer LeGentil was near Madras for the event; but on the day before the transit, he was kept from getting ashore by an English man-of-war and he missed the spectacle. The later Astronomer Royal, Nevil Maskelyne, did observe it from St. Helena, and Charles Mason and Jeremiah Dixon were sent out by the Royal Society in London for the same purpose. The latter pair, however, soon after getting underway, ran into a French frigate, which encounter left 11 English sailors dead and 38 wounded. That was too much for Mason and Dixon, and they determined to call an end to the adventure. The Royal Society considered the possible embarassment to the country at large and to the Society in particular, and com

municated their concern to the frightened pair in strong terms. They acquiesced and went off to observe the transit.

By 1769 we find LeGentil still in India, where he had in the interim founded a successful business in order to support himself. The day of the transit found him happily setting up his instruments, ready to observe in the most beautiful weather available anywhere in the world, and in particular during a season of clear, cloudless weather there. Just before the transit, clouds formed; it rained and continued raining until the transit was over, whereupon the rain ceased and the skies cleared. Meanwhile, Captain James Cook enjoyed the opposite fortune in Tahiti, where he had established two observing posts for the event. Observations were also made in American colonies by its amateur scientists. Their reports were as good as (or no worse than) those made by professional astronomers enjoying official patronage.[4]

The question of the Sun's distance was not settled by the 18th century transits, and the solar parallax still awaited a good determination when in 1874 numerous countries sent out expeditions to remote parts of the Earth to observe the first of the 19th century pair. Details of the general preparations made by the United States are treated elsewhere.[5] It is important to note that plans were made at the last minute and that the U. S. effort differed in another important respect from others in the sense that emphasis was to be placed on photographic observations. This must have been considered by many to be foolhardy; photography had not yet been accepted for any serious scientific purpose. The idea was nevertheless adopted by the Commission on the Transit of Venus, which drew to itself some of the brightest minds of the age: Joseph Henry, Benjamin Peirce, Simon Newcomb, William Harkness, George William Hill, Asaph Hall, Henry Draper and innumerable others, including scientifically minded Superintendents of the Naval Observatory.

The plans of all national expeditions had at least one common feature. Detailed reports were to be made not only concerning the minutest details of the observations but any other scientific knowledge obtainable as well. The reports were eventually published by the various countries which took part. In some instances, these ran to several volumes whose considerable bulk is impressive. The United States was to publish its complete report in three or four volumes. Volume I has actually been printed, but it contains only a summary.

At this point, I would like to engage in a kind of travelogue, in which reliance will be placed on the unpublished records of the Transit of Venus Commission and on photographs provided by descendants of some of the principals.[6]

Fig. 1. Part of the 1874 Transit of Venus party at Vladivostok, Russia. The figure at center is Asaph Hall, chief of the party.

Fig. 2. Vladivostok with some of the Transit of Venus sheds in the foreground.

The northernmost site occupied by the United States expedition was at Vladivostok, Russia. Figure 1 shows some of the observing party. In Figure 2 we see the town itself. With a little effort, one can make out the equatorial house and the arrangement of the photograph sheds, to be seen more clearly in later photographs. The ship in the background is probably the *U. S. S. Kearsarge*, which delivered the party.

Figure 3, from Nagasaki, gives more detail concerning the photographic apparatus. The instrument was designed as a reflecting telescope of some 40-feet focal length, the focus being at a photographic plate holder inside the shed. Photographs, when taken, were immediately carried into an adjoining room of the shed for developing. The barrels seen in the figure contained water for the photochemicals. Figure 4 shows the heliostat part of the photographic telescope. The mirror is in place, and is nothing more than two clear glass plates set at an angle, so as to reduce the amount of sunlight and to eliminate multiple reflections. What at first appears to be a pile of trash in Figure 5, is on closer inspection an exhibit of the appliances used to support the developing and fixing of the photographic plates. There also can be seen the photographic house and, to the right, the iron gas pipe used to measure the focal length of the heliostat. The familiar instrument in Figure

Fig. 3. The Nagasaki observing site, with the heliostat visible at left and the photographic house at right.

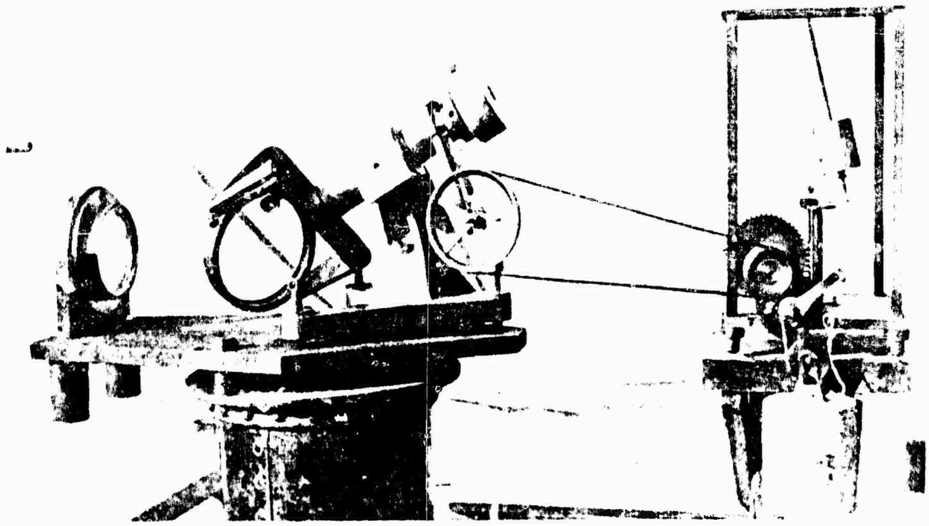


Fig. 4. A close-up of the heliostat at Nagasaki.

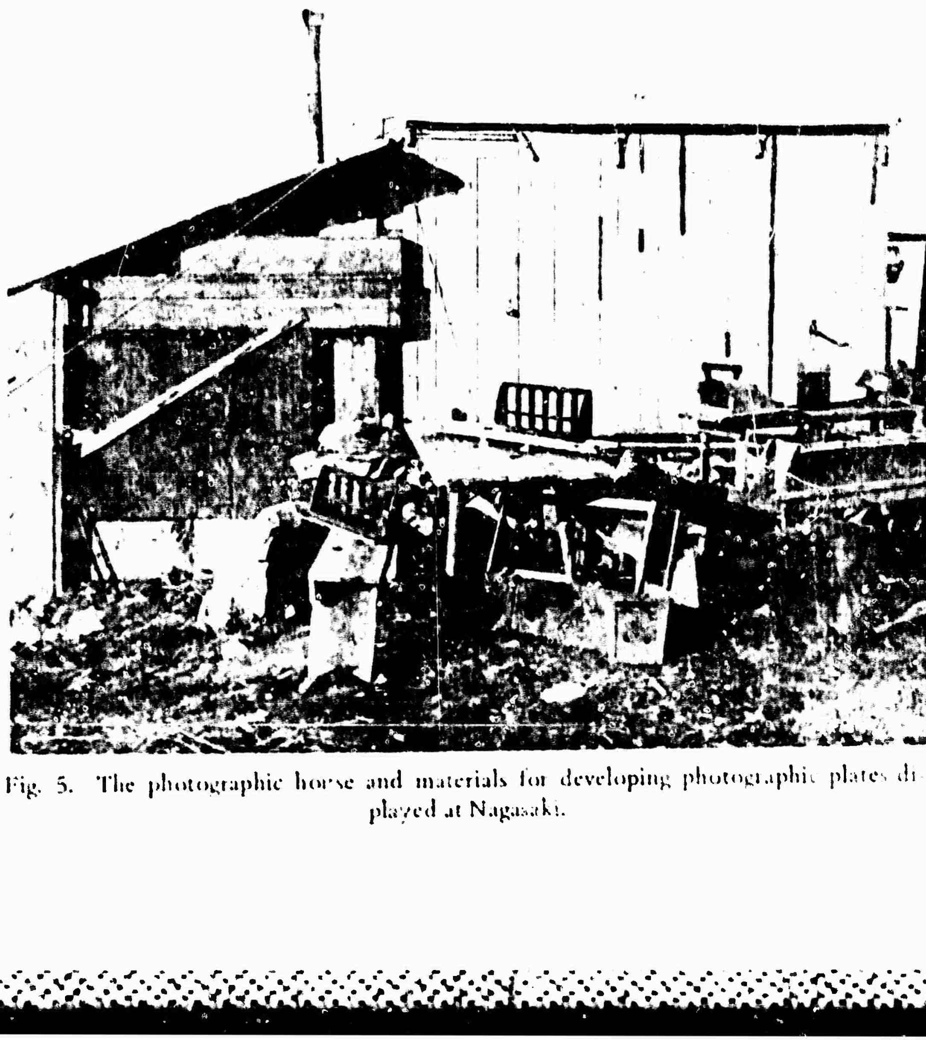


Fig. 5. The photographic horse and materials for developing photographic plates displayed at Nagasaki.

Fig. 6. Clark refractor at Nagasaki.

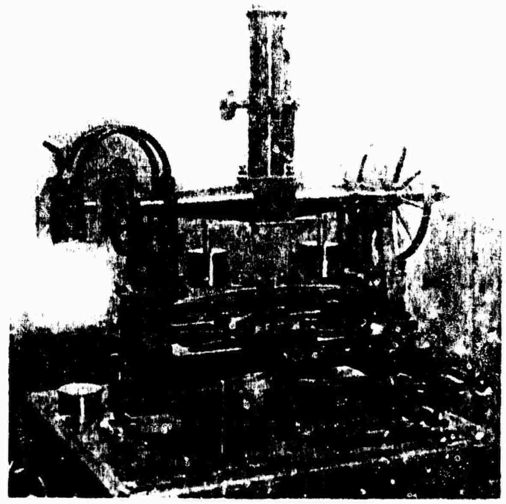


Fig. 7. Stackpole "broken" transit instrument.

6 is a five-inch Clark refractor, equatorially mounted. The United States' party did not neglect visual timings of the event, despite the emphasis on photography, and the five-inch telescopes were the official instruments for that purpose.

Much of the value of any observations depended upon accurate knowledge of the site location. Since the geographic coordinates of remote locations were known crudely at best, it was an integral part of the mission to determine coordinates by celestial observations. For that purpose, each party was equipped with the required instruments: a clock and a "broken" transit (telescope). The latter is shown in Figure 7. It appears to have been a handsome instrument. The observers were rather unhappy with the performance of the transit and none of the instruments are known to have survived their original form to the present.[4] Some observers took along more reliable instruments which they obtained at their own initiative. The Nagasaki party did not neglect the beauty of the country, and Figure 8 is a view of the harbor area. An interesting detail in the photograph is a telegraph pole. George Davidson, in charge of the party, was a strong proponent of the telegraphic determination of longitude, and he engaged this method while at Nagasaki.

[4] Note added in 1983: Two of the broken transit instruments have now been found and are in the historical instrument collection of the Naval Observatory.

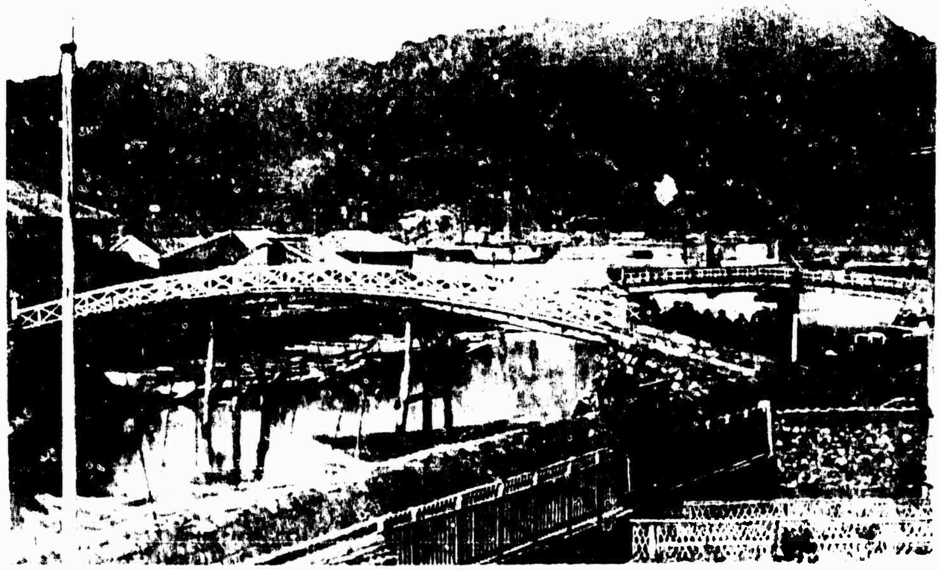


Fig. 8. View of Nagasaki harbor.

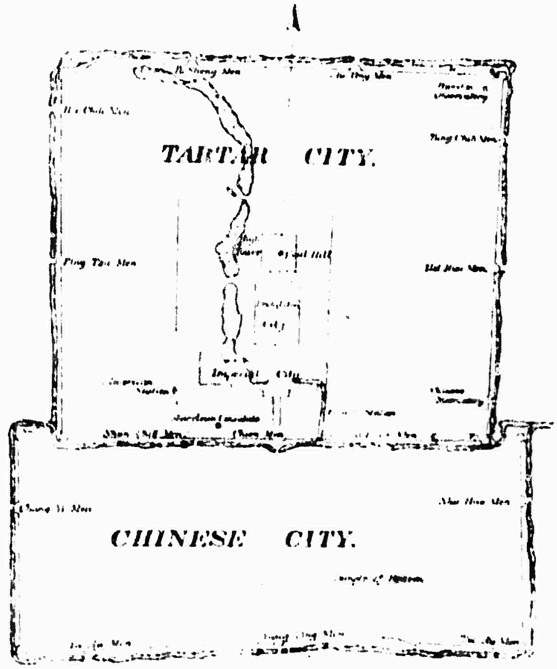


Fig. 9. Sketch map of Peking, China, site of the third northern hemisphere U.S. station.

Fig. 10. Barren scene from Kerguelen Island in the Indian Ocean.

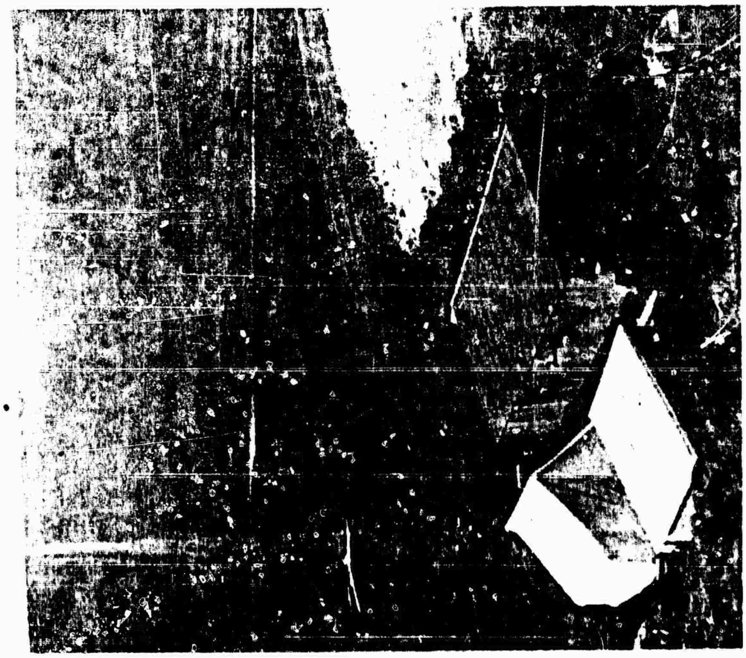


Fig. 11. Transit of Venus station on Kerguelen Island.

Fig. 12. Kerguelen Island: Captain Cook's "land of desolation."

Fig. 13. Wind-pollinated cabbage on Kerguelen Island.

The history o. efforts to fix longitude telegraphically is incompletely known and deserves comprehensive study.

We stop briefly at the capital city Peking, where James C. Watson and his party worked. Figure 9 is one of the few graphic mementos of their visit. Perhaps the taking of informal photographs was frowned upon. On the day of the transit, when a spot appeared upon the Sun, a mark appeared upon the face of the Chinese Emperor. This last mark was symptomatic of something quite seriously wrong with the Emperor's health. Although a youth, he lacked sufficient vigor to cope with the illness and he succumbed. There was enough superstition among the people to associate the two events. Moreover, power was taken by the Regent. As the Americans viewed both developments, hasty withdrawal seemed their prudent alternative. In this they were assisted by the French party, which remained in Peking and suffered no ill consequences.

The three stations mentioned were the extent of the United States' commitment in the Northern Hemisphere. Five groups were sent to the Southern Hemisphere. The remaining photographs which I have obtained have come from (unofficial) efforts of the photographers assigned to the southern parties. Many of the photographs were to make public appearances as stereopticon slides, reproduced and offered for sale by the same photographers. Irvin Stanley was at Kerguelen Island in the South Indian Ocean. He and W. H. Rau, who was at Chatham Island, exchanged copies of their work. During the 18th century, it was usual for voyages of exploration to have artists and naturalists among the ships' complement. Sketches, drawings and paintings returned with the adventurers and enhanced their reports. Similar opportunities were lost in 1874 when most expeditions were ill-equipped for photography.

Figure 10 (like most of the remaining slides) was copied from a stereopticon slide having a sepia-like tone. It is a scene from Kerguelen Island. It looks grim, foreboding, barren, and according to first hand accounts, satisfied those qualities in fact. Figure 11 is the transit party camp. A more typical scene is that in Figure 12. The place was described by Captain Cook as a land of desolation. It became more so after Cook left it, for his maps were used by others who later decimated the wildlife. Among such commercial adventurers were whalers, who supplemented their diets by the cabbage shown in Figure 13. This is an interesting plant, for it is wind pollinated. It had to be thus, since there were no winged insects on the island.

Chatham Island was at least inhabited. Figure 14, taken there, was annotated "Waurekauri, 14 miles from camp" by Rau. In Figure 15, Bill Lennent, a whaler shipwrecked in 1842, is shown with his Maori wife. Conceivably,

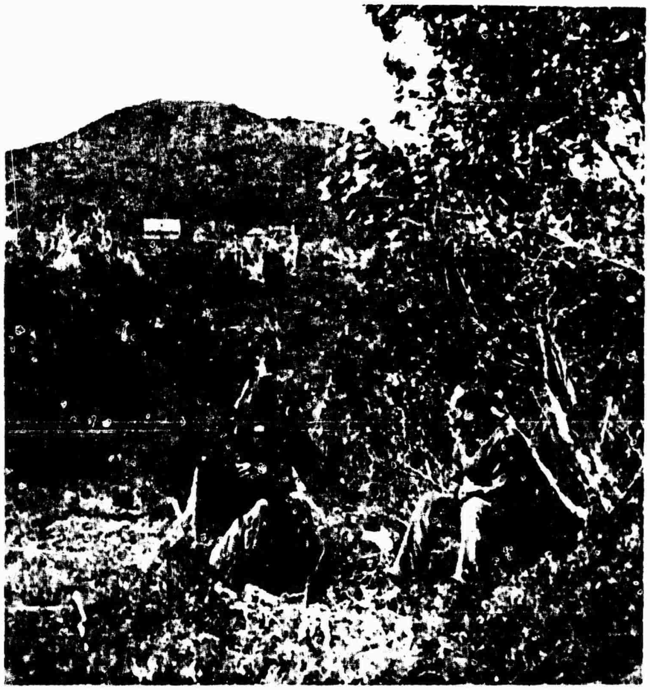


Fig. 14. W. H. Rau's photograph of "Waure-kauri, 14 miles from camp," Chatham Island in the South Pacific.

Fig. 15. The American whaler, Bill Lennent, with his Maori wife, on Chatham Island.

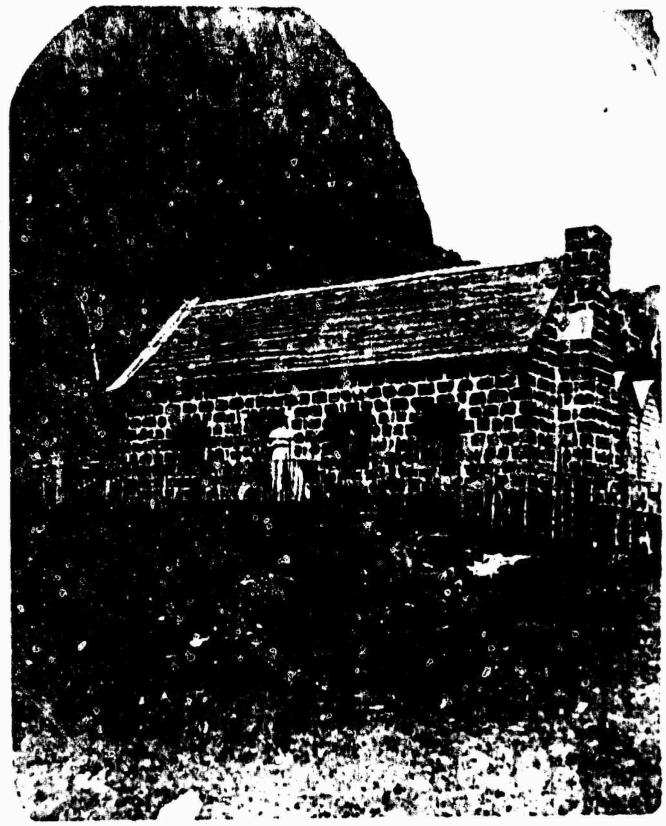


Fig. 16. Residence of Mr. Ernst, a missionary, about nine miles from the observing site on Chatham Island.

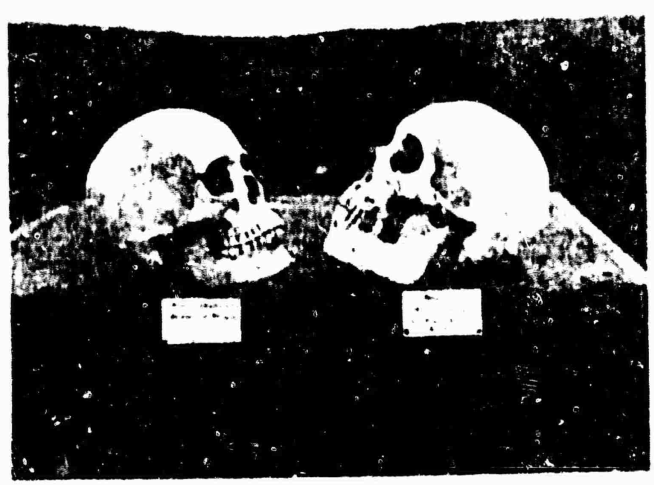


Fig. 17. Skulls of "Moriari, aborigine of Cnatham Island" and of a Maori, Chatham Island, "devourer of Moriari."

Fig. 18. Lake Wakatipu, from the observing site near Queenstown, New Zealand.

Fig. 19. Bullerat Street, central town area, Queenstown, New Zealand.

they were married by Mr. Ernst, a missionary who arrived in 1844 and whose residence is shown in Figure 16. Gripping contrast to impressions of pastoral serenity is provided by Figure 17, which reminds us of a dark history. The crude, clinical arrangement of skulls is augmented by cards offering a terse explanation. One reads, "Moriari, aborigine of Chatham Island"; the other states simply, "Maori, Chatham Island, devourer of Moriari."

If global weather conditions affecting all expeditions were to be summarized in two words, those might be "generall[illegible]r." A notable exception was enjoyed by C. H. F. Peters and his party i[illegible] Zealand. It was widely reported that the weather there had been so bad that no one in New Zealand had seen the event. Peters located in Queenstown, on the shore of Lake Wakatipu, where he in fact enjoyed ideal weather. From his observing site comes Figure 18, showing the lake and mountainous surroundings. The town is not evident in this photograph, but some idea may be had from Figure 19, which is a part of the central area. One may imagine that Peters' party enjoyed their stay; Queenstown prospered to become a favorite South I land resort. All this, and science too.

Searching for the actual transit plates was to become a frustrating experience. Some have turned up recently, although it is not certain that they came from the 1874 expedition. Figure 20 is typical. Venus is at the top (the

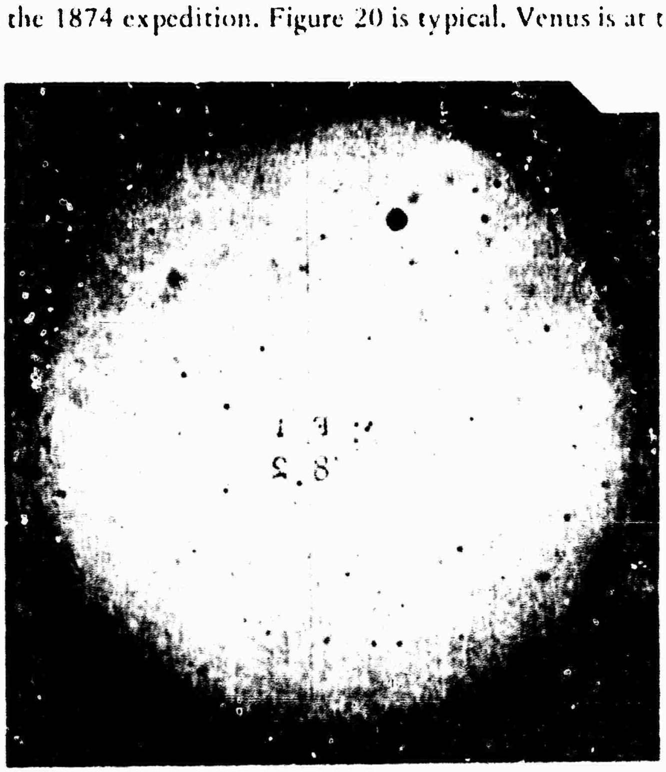


Fig. 20. Transit of Venus plate.

other marks are not necessarily sunspots). A reseau, or network of parallel lines, was imprinted onto the glass plate. The heavy line through the center is the shadow of a plumb line used to orient the plateholder and image with respect to the vertical.

Interest in Venus transits for determining the solar parallax waned after 1874. Except for the United States, all countries had relied upon visual observations of contact between the limbs of Venus and the Sun. As in the case of the 18th century transits, these were widely discordant. Improved instrumentation and practice with foreknowledge of physiological effects had not produced the hoped for advantage. Disappointment was widespread, and the whole enterprise deemed generally a failure. Of the United States' plan and its validity we cannot know. Certainly there were many painstaking details which had to be reckoned with regard to the radical application of photography; there were determinations of instrumental errors to be made; geographic locations precisely wanted. There was a considerable effort made at first. In view of all the difficulties, mostly not of a scientific or procedural kind, the U. S. effort could not be sustained, while the years between 1874 and 1882 saw preference being shifted to other methods for determining the parallax. By the time of the 1882 event, the level of urgency had declined to the point where expeditions were sent principally for the reason that it was the thing to do. Of course the skies were then propitious for nearly every observer.

One man never lost faith in the value of the 1874 photographic observations. Knowledge of him today is as faded as the detailed history of the U. S. expedition. He is William Harkness, who eventually became the Astronomical Director of the Naval Observatory. His accomplishments were many and varied. Among them, the spherometer caliper, an indispensible tool in instrument and machine shops, he invented to measure pivots of transit instruments. He participated in the design of the six-inch transit circle at the Observatory, and was intimately involved in the Observatory's relocation in 1893.

For several years, Harkness labored alone over the reductions of the transit observations. Although he was overtaken by other duties and cares, there is some evidence that the completion and publication of the results of the 1874 expedition were goals never far from his mind. Harkness (Figure 21) deserves to be better known. I hope that an enlightening or definitive study of Harkness will someday be written. As source material, there are four volumes of Harkness' letters and scientific writings at the Naval Observatory, which itself stands as a part of his creative output. The archives at the University of Rochester is another repository.

Fig. 21. William Harkness (1837-1903), Naval Observatory astronomer, 1862-1899, and first Astronomical Director, 1894-1899.

In closing, I would like to read part of a paragraph from an address given by Harkness before the American Association for the Advancement of Science, prior to the transit of 1882.

> We are now on the eve of the second transit of a pair, after which none other will occur until the 21st century of our era has dawned upon the Earth and the June flowers are blooming in 2004. When the last transit season occurred, the intellectual world was awakening from the slumber of ages, and that wondrous scientific activity which

> has led to our present advanced knowledge was just beginning. What will be the state of science when the next transit season arrives, God only knows. Not even our children's children will live to take part in the astronomy of that day. As for ourselves, we have to do with the present, and it seems a fitting occasion for noting briefly the scientific history of past transits . . .

We are in a slightly more advantageous position to speculate than Harkness or his contemporaries. Their "children's children" have left footprints on the Moon, safely navigated the asteroid belt, and tested Venus' surface, so that quite likely the next transits of Venus will be but astronomical curiosities.

It is more appropriate to comment on the significance of the U. S. participation in the 19th century events. The federal establishment had, at that time, a tradition of not directly engaging in science, unless it seemingly could not be avoided. Since other nations were committed to field expeditions in large number, it could have been argued that the United States need not; rather, it could employ the resultant solar parallax to practical requirements when it became available in the literature. The government's slow arousal and hectic, eleventh hour preparations may be interpreted as testimony of a strict constructionist attitude. Yet, the eight parties which were dispatched were thoroughly staffed, trained and equipped. Among the great nations at that time, the United States was not a scientific leader. Had circumstances been kinder and, in particular, had it been possible to complete the results, the 1874 expedition could have significantly advanced the time when scientific leadership by the United States was acknowledged.[7]

Q: Were values for the parallax ever derived from the 1874 results?

Janiczek: There was a value published by D. P. Todd, I believe. It was premature since the reductions had not been finished. The 1882 transit results were printed, as far as I know, only in a report of the Secretary of the Navy for 1889, and I suppose that satisfied the requirements of the work in a legal sense, although certainly not in a scientific one.

Q: I've been told that in 1879 there was a publication in the Congressional series on the 1874 transit produced by Newcomb with values of the parallax.

Janiczek: That had to be a preliminary value. Newcomb himself claimed the work was never finished.

Fig. 22. "What will be the state of science when the next transit season arrives, God only knows. Not even our children's children will live to take part in the astronomy of that day. As for ourselves, we have to do with the present. . . ."
(Illustration from *Harpers Weekly*, April 28, 1883.)

NOTES

1. For a somewhat more detailed account of this subject, see P. M. Janiczek and L. Houchins, "Transits of Venus and the American Expedition of 1874," *Sky and Telescope*, 48 (December 1974), 366-371.

2. The title of the complete work is *Observations of the Transit of Venus, December 8-9, 1874, Made and Reduced Under the Direction of the Commission Created by Congress*. Part I, "General Discussion of Results," was printed bearing the date 1880. The document referred to here is Part II, "Observations," of which only a single copy exists as two bound volumes of page and type proofs. Part III, containing the discussion of the longitudes of the stations from occultations and other sources, and Part IV, containing the photographic plate measures, have not been found in any form.

3. One would like to know more about Horrox with greater accuracy. I have not been able to examine any of his writings except via quotations embedded in works which are less than scholarly and which, together, lead the reader to some contradictions.

4. Literature describing the 18th century transits is not wanting generally. The work by Harry Woolf, *The Transits of Venus* (Princeton, 1959) is an authoritative account. Newcomb, in *Reminiscences of an Astronomer* (Boston and New York, 1903), highlights several of the misfortunes, while devoting Chapter 6 to the subject in general.

5. E.g., *Papers Relating to the Transit of Venus in 1874* (Washington, D. C., 1872).

6. Figures 1-9 were found among various collections at the Naval Observatory, as were Figures 18-20. Ruth Haskett Stines, author, genealogist and descendent of Irvin Stanley, one of the expedition photographers, has provided Figures 10-17 and many others, including some from the 1882 transit. Marshall Pywell, a descendent of William Reddish Pywell (the transit party at Campbelltown), kindly provided Figure 22.

7. W. D. Horigan assembled a typewritten 30 page "Memorandum for the Superintendent (Naval Observatory) concerning the origin and operations of the U. S. Transit of Venus Commission" in 1922. It is comprised chiefly of extracts from reports by the Commission and the Secretary of the Navy. It also reproduces a letter, dated 1891, by Harkness indicating his intentions to complete the work of the transit expeditions.

Transit of Venus Commission and Field Expedition Members at the "Old Naval Observatory," spring 1874. Standing at the left, with his arm on the heliostat, is RADM C. H. Davis; beside Davis, hatless, is Henry Draper; behind Draper's left shoulder is C. H. F. Peters; seated on the camp stool is Simon Newcomb, Chairman of the Commission; standing in front of the ladder, arms folded, is Asaph Hall; seated at the extreme right is Irvin Stanley. Identifications were made with the assistance of J. K. Herman, Navy Medical Command.)

Simon Newcomb (1835-1909) held positions in the Nautical Almanac Office, 1857-1861, and the Naval Observatory, 1861-1877, before serving as Superintendent of the Nautical Almanac Office, 1877-1897.

SIMON NEWCOMB'S ROLE IN THE ASTRONOMICAL REVOLUTION OF THE EARLY NINETEEN HUNDREDS

Arthur L. Norberg
National Science Foundation*

Introduction

In honor of the 150 years of the U. S. Navy's role in astronomical affairs, through its many modifications from a depot of charts and instruments to the present form of the Naval Observatory, I would like to focus here on one context of its activity: the development of planetary tables and position constants. The research for these tables and constants was performed in the Nautical Almanac Office at a time when it was independent of the Naval Observatory, but in the same administrative branch of the Navy.

The greatness of institutions such as the Almanac Office can sometimes be appreciated through the greatness achieved by persons active in those institutions. In the case of the Almanac Office, we can appreciate a part of its greatness through a study of the efforts of Simon Newcomb. Research efforts on planetary table theory reveal Newcomb's role–and the Navy's part–in the opening salvos of the astronomical revolution of the 20th century, a revolution that is still unfolding. And, in keeping with this celebration, the context illustrates one of the significant areas of the Naval Observatory's and Nautical Almanac Office's contributions to this revolution.

The research program on constants and tables at the Almanac Office in the last third of the 19th century was led by Newcomb. The task was an immense one, and involved the cooperative efforts of many astronomers throughout the world. Newcomb's planning for and coordination of this work brought him greatness. When contemplating how to relate briefly the details of this important program so as to justify his colleagues' view of Newcomb's greatness, and feeling overwhelmed by the amount of detail, I was reminded of a comment by Charles Sanders Peirce on greatness. He wrote:

> . . . the way to judge of whether a man was great or not is to put aside all analysis, to contemplate attentively his life and works, and

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> then to look into one's heart and estimate the impression one finds to have been made. This is the way in which one would decide whether a mountain were sublime or not. The great man is the impressive personality; and the question whether he is great is a question of impression.[1]

Let me, then, offer such an impression by concentrating, first, on the outline of the plan and, second, on the reaction of astronomers to the research results. Even this brief reconstruction reveals several things about Newcomb that contributed to his greatness: his intellectual prowess, the force of his personality, his organizational ability, his view of scientific research, and the attitudes of his contemporaries toward him. At the same time, it illustrates a significant contribution of the Almanac Office and the Observatory.

Newcomb's Research Program

The basis of all measurement is an accurate knowledge of positions; yet throughout astronomy's past, no tables represented the positions of bodies accurately for very long. In order to determine whether this difficulty is the result of theory or observation, it is necessary to have a good set of tables for comparison, and to refine the reduction of observations as much as possible. Addressing himself to this point in 1882, in the first announcement of the design for carrying out his planetary program, Newcomb stated that,

> If we are to determine what unknown causes affect the motion of the planets the first step is to prove that there is really a discordance between the results of observations and the results of the theory of gravitation. The first step toward establishing such a discordance is the construction of tables and formulae of which we can say that they are beyond reasonable doubt the results and only results of the gravitation of the known bodies of the solar system. The necessary conditions which such tables and formulae must satisfy are that they shall be founded upon uniform elements and data, and that the results of employing the adopted elements shall be carried out with all necessary precision. Now, not only has this requirement never been fulfilled, but the effect of recent advance in exact astronomy has rather been to carry us away from its fulfillment.[2]

Newcomb criticized Urbain Leverrier's work, the most recent attempt to provide a set of tables, on the grounds that he pushed the tables into print too fast and did not use all the information available to him. This convinced

Newcomb that just improved tables should not be the only goal. In order to accurately determine the fundamental elements of astronomy, it was necessary to use all the material available, which had "increased many fold" since Leverrier's tables appeared. He noted that,

> The desirableness of having such tables founded on one consistent and fully elaborated theory, hardly needs to be insisted upon. Only in this way can it be decided whether deviations from theory arise from its imperfections, or from the actions of unknown and, perhaps, unsuspected causes.[3]

Newcomb was concerned, because if Leverrier's tables became the standard a great part of the existing data was "in danger of never being used, unless discussed and condensed, in such a way as to render them manageable." This referred to the observations taken particularly after 1860.

> We must either suffer this great mass of materials, collected at great expense, to go to utter waste, or we must speedily put it in a shape to be utilized for present and future purposes. It is true that if nothing were to be added to the mass we might safely leave it in confidence that future astronomers would give it more attention than we have. But so rapidly does it increase . . . the required work must be that of an organization rather than that of an individual.[4]

He insisted that all the work should be founded on all the observations available, but, in addition, on all the available discussions by other astronomers. This would reduce its individual character, and allow a greater utilization of material.

Since a description of the orbits followed by the four inner planets required most all the fundamental constants of astronomy, Newcomb proposed to begin with these planets as a set. In these interactions, one needed the masses of the planets and the elements of their orbits, the constants connected with the rotation of the Earth, the annual precession, the obliquity of the ecliptic and its secular variation, the position of the equinox, and the positions of the fundamental stars; one needed also the solar parallax and the mass of the Moon.

Newcomb thought that the theoretical aspects of these investigations would have more lasting value than the tables. It was this conclusion that stimulated Newcomb to develop his theoretical analysis. For this he established a new series within the Almanac Office to handle the publication of

his results, the *Astronomical Papers Prepared for the Use of the American Ephemeris and Nautical Almanac.*

We get indications of his desires much earlier than 1882, however. Shortly after Newcomb assumed direction of the Almanac, he began restructuring the office. In the annual report of the office for 1877, written by Newcomb only one month after assuming the Superintendency, he wrote that,

> The most urgent want of the office at the present time is a set of tables of the Moon and planets, corresponding in accuracy to the present state of practical astronomy, and founded on entirely homogeneous data.

To justify this statement, he commented on the tables in use in the following way.

> The tables of Mars, Jupiter, and Saturn, now used in the preparation of the *Ephemeris* are all more than half a century old, and the only recent ones existing are those of Leverrier. These have never been introduced in the preparation of the *Ephemeris* because their form is such as to render them extremely inconvenient in use, and it is doubtful whether they fulfill the requirements of the astronomy of the present day in respect to precision.[5]

He wrote this at a time when the ephemerides of Europe were contemplating the introduction of Leverrier's work. Newcomb finished the report by asserting that if the whole appropriation for the next fiscal year were approved, "the object in question can be commenced as soon as that appropriation is available."

On May 15, 1878, Newcomb wrote to Alexander Agassiz, in Cambridge, Massachusetts, that he was "getting fairly under way" the theories of the planetary motion.[6] One of Newcomb's first steps was a proposed revision of the *Ephemeris*, to improve its usefulness, and to free some time of the computers for other work. He submitted to the astronomers of the country fifteen suggested changes, that ranged from the omission of all data for Washington mean noon, to the addition of heliocentric longitude, latitude, and radius vector values for all the planets. Differences of opinion arose on some of the points, and the proposed changes were referred to a committee of the National Academy of Sciences. This committee sustained most of the recommendations and suggested modifications of the others,[7] which Newcomb incorporated in the volume for 1882, published in [illegible] 18[illegible].

The new structure of the *Ephemeris* required less calculation, so the office personnel could spend some of their time on the new planetary program. In addition, Newcomb hired some new help, with funds from an appropriation for the computation of tables for planets (i.e., minor planets) discovered by American astronomers. Newcomb interpreted the terms of this appropriation rather broadly, and used some of the funds for tables generated by American astronomers, regardless of whether the planets were discovered by them. Eventually, he successfully lobbied for an appropriation to cover the planetary table work itself. The original amount of this latter appropriation was \$3,000, and over the next 15 years it fluctuated from \$2,000 to \$9,900 per year.

One regular assistant of the office, George W. Hill, was assigned the work of Jupiter and Saturn. He coordinated this work with Newcomb's intentions for the entire program, but, essentially, he was free to generate the tables in his own way. This was a massive task and it took Hill ten years to complete it. Newcomb divided the rest of the work into two parts: the inner planets, as we noted above, and Uranus and Neptune; but, for the motion of these last two planets Newcomb felt that only updating of his earlier tables would be required when the rest were completed.

Newcomb's program for the analysis of the motions of the inner planets can be summarized in the following way.[8] First, he discussed the computation of the general perturbations of the planets. For the four inner planets, fourteen pairs of planets had to be considered, because of the effects of Jupiter and Saturn on the inner planets, as well as their own interactions. One important element in sorting out the perturbations of the planets on each other was the mass of Jupiter. To determine this value, Newcomb used the interaction between Jupiter and the asteroid Polyhymnia, discovered in 1854. This asteroid made a near approach to Jupiter in 1885, and reached perihelion in 1888, so Newcomb was able to use observations of it from 1854 to 1889. Second, he undertook the re-reduction of the older observations, and a discussion of the later ones to reduce them to a uniform system. He re-reduced Maskelyne's Greenwich observations from 1765 to 1811, using modern data; and he did the same for Piazzi's observations at Palermo for 1791 to 1813.

To save time, he carried over Auwer's reductions of Bradley's observations of 1750 to 1762, and Airy's reduction of Pond's observations of 1812 to 1830, as well as Leverrier's reduction of the Paris observations from 1800 to 1875, and Bessel's reduction of the observations at Königsberg from 18[illegible] to 1845. To these Newcomb added his own reduction of later observations made at over a dozen observatories.[9] The total number of observations ulti

mately used for the four inner planets was 62,030.

Third, since Leverrier's tables were in use in the ephemerides of Europe from 1864 on, Newcomb decided to calculate places for the planets prior to 1864 from these tables also. He began with Leverrier's formulae, partially reconstructed them, and recomputed the positions of the four inner planets and the Sun. From these manuscript tables, Newcomb calculated a set of elements, which he employed as the provisional elements in the development of his own theory. Several supplementary tables were also generated, such as those for changing the Sun's positions in longitude and latitude to right ascension and declination, and changing the latter from Greenwich mean noon to Greenwich apparent noon and then to apparent noon at the observatories whose observations were to be used. His final results embraced a complete revision of the elements of the planetary orbits and the masses of the planets, as well as the principal constants of reduction, such as solar parallax, precession, aberration and nutation.[10]

Newcomb combined exceptional talent for reducing observations to a uniform system, thereby increasing their utility and reliability for empirical correction of the elements of an orbit, and reducing the equations of condition by least squares so that they contained a minimum of error. Nevertheless, sometimes he placed too much faith in the work of another, and errors crept into his final results.

The problem of observations, with respect to both number and quality, was perhaps more severe than that of the development of theories for the planets. In order to reduce these observations for use in the equations of condition, Newcomb had to adopt a highly accurate reference system of stars. Although pleased with the work of Arthur Auwers at Berlin, who assembled a list of the right ascensions of reference stars for the epoch 1870, at the behest of the Astronomische Gesellschaft, for use in the *Berliner Jahrbuch*, Newcomb noted that the proper motions used in the *Jahrbuch* were systematically slower than the motions of the stars used in the *American Ephemeris* by 0.08 seconds per century.[11] Therefore, Newcomb developed his own fundamental system for the right ascensions,[12] which included all the standard stars of the *American Ephemeris*, and all the stars, down to the sixth magnitude, that could be occulted by the Moon. For the declinations, he adopted the system of Lewis Boss, of the Dudley Observatory at Albany, New York, which Newcomb had incorporated into the *Ephemeris* in 1881.[13]

The Reaction of the Astronomical Community

Newcomb constantly corresponded with Lewis Boss, concerning the standard star system to be adopted, and with David Gill about the value of

heliometer observations, and he also encouraged and instructed both Boss and Gill concerning the type and quality of observations he desired. On February 28, 1891, Newcomb wrote Boss,

> In about two years . . . my planetary work will be so far advanced that I shall have a corps of computers ready for something else. The most urgent work will be the preparation of a standard catalogue of perhaps 1,000 standard stars for use in the *Ephemeris*. I have all the machinery necessary for the preparation of this catalogue . . . Whatever is done, we should work together . . .[14]

Arthur M. W. Downing, Director of the British Nautical Almanac Office, wrote Newcomb in June 1894,

> I am glad to hear of the progress of your new Solar Tables, which will, I hope, be in a sufficiently advanced state to allow of their introduction into the *Nautical Almanac* for 1900.
>
> It will be a great advance when astronomers are able to compare their observations of the bodies constituting the solar system with tables deduced from absolutely uniform astronomical data, and I look on your Solar Tables as a first step in this direction. I hope you will be able to bring your work to a successful conclusion, and to publish it without any unnecessary delay, as the tables will undoubtedly prove of great service to astronomy in general, and to such an office as this in particular; and the sooner they are made available for use, the better for science.[15]

One year later, Downing agreed with Newcomb's plan to prepare a standard star catalogue for 1900, and expressed "no doubt" that it would serve as a basis for an international catalogue.[16] Shortly after, David Gill began a movement to stimulate cooperation among the ephemeris offices, by proposing, in the spring of 1894, that a conference be held in 1896 to discuss common problems and to work out a unified basis, like Newcomb's, of constants that could be used in all almanacs.[17]

The reaction to Gill's proposal was not overly warm. Newcomb welcomed it for obvious reasons, and, since the British and United States offices were already discussing cooperation, Downing blessed the idea. The Germans (i.e., Arthur Auwers) frowned on the proposal, and the French took a wait and see attitude. After a flurry of letters, and a watering down of the aim of such a conference, Downing, on January 28, 1895, wrote to Maurice Loewy,

Director of the *Connaissance des Temps*, suggesting that it would be advantageous for the principal ephemerides to adopt a unified standard star system.

Invitations to the conference meeting in Paris were limited to the directors of the four principal ephemerides, who could choose one other participant, and three consultants who represented observatories, to give a total membership of eleven. This organization permitted Gill to represent the southern observatories, and allowed Newcomb to nominate O. Backlund, Director of Pulkova, who otherwise would have been excluded.

When Newcomb left for Paris, his program on the constants and the tables of the four inner planets was partly published and the rest was in the hands of the printer. The report on the investigations into the astronomical constants appeared toward the end of 1895; the tables of the Sun, Mercury and Venus were in press; the Almanac Office had begun to assemble a "consistent list" of stars, for a new standard catalogue.[18] Naturally, Newcomb felt that this body of work contained the highest accuracy obtainable, and he went to the conference armed with a proposal that, if accepted, would cause Newcomb's constants and tables to be introduced into the world's ephemerides.

At the opening session, Loewy suggested that Newcomb present a proposal on the procedures to be followed, and the aim to be sought by the conference, because of the "great quantity" of his investigations relevant to the questions to be considered. Newcomb presented seven propositions.

The participants discussed these proposals. Tisserand, Director of the Paris Observatory, recommended that the catalog discussed should be an all purpose catalog of right ascensions and declinations, rather than recommending the publication of each coordinate separately, as had been done in the past. Downing futher specified that the conference should not only discuss the constitution of the catalog, but also set up the machinery of its production. At this point, Gill tried to add to the agenda a discussion of what tables of the Sun and the planets would be most suitable for common adoption, but the members sidestepped the issue as "extremely delicate" and "very difficult to resolve." No wonder the choice was either Leverrier's tables, in use over Europe, or Newcomb's forthcoming set. The final program decided on for discussion included Newcomb's seven points, modified by Tisserand's proposal for a more general catalog, Gill's requests, and a suggestion by Julius Bauschinger, Director of the *Berliner Jahrbuch* to attempt to organize a basis for calculations of the ephemerides of small planets.

The Revolt of the American Astronomers

On Newcomb's return to the United States, he tendered a report of the conference to the Secretary of the Navy. He began to smooth the path for adoption into the *American Ephemeris* of the constants proposed by the conference, by inserting the following statement into his report:

> In a general way it may be said that the elements adopted were founded mainly on the researches made in the office of the American Ephemeris during the past fifteen years. The adopted system of Right Ascensions of the Fundamental Stars, on which all others are to depend, is that published in Volume 1 of the astronomical papers of the American Ephemeris (*sic*). The most noteworthy deviation from the numbers adopted in this office was that the values for the three constants of Nutation, Aberration and Solar Parallax were those derived directly from observation, without making any adjustments for the theoretical relations existing between them. A slight difference thus arises between the adopted values and those adopted by the office in the planetary tables; but the difference is so slight that I have deemed it advisable to accede to the conclusions of the Conference so far as the Ephemeris of the Fixed Stars is concerned.
>
> The International Catalogue will, it is expected, be one now being prepared at this office; and I have undertaken to redetermine the constant of Precession for subsequent international use.[19]

Newcomb set to work immediately on the tasks assigned to him by the conference. His new analysis of the precessional constant appeared in the *Astronomical Journal* in June 1897.[20] Newcomb chose his 1895 value as the initial constant needing correction. The equinox to which Newcomb compared this value possessed a right ascension from his 1882 catalogue, and a declination from a new fundamental system, constructed for the purpose. The stars used in these systems came from Auwers' reductions of Bradley's observations, employed by Newcomb because Auwers had calculated the proper motions.

This study appeared shortly after the *Procès-Verbaux* of the conference[21] became available, and American astronomers were grumbling about the attempted imposition of the results of the conference on all astronomical activity. A few saw the role of the almanac as a service organization, and they felt that any changes should come about only because the community desired them, rather than through the decision of the directors of the almanacs. Furthermore, by Naval requirement, Newcomb retired on March [illegible],

1897, at age 62, so there was some discontent that he tried to incorporate the new constants without consultation. Nevertheless, in spite of the discontent, it appears that Newcomb never lost confidence of ultimate adoption, but his relationship with his successor, William Harkness, with whom he had had several encounters over the years, was difficult at best.

A running controversy developed in the pages of the *Astronomical Journal*, with publications principally by Lewis Boss, the editor, and individual pieces by G. W. Hill and Seth C. Chandler.[22] First, Boss raised the issue as to whether the decisions of the conference held for all users of the almanac, or just certain branches, such as the naval services and the merchant marine. If, he argued, all the ephemerides adopted the constants, no alternative would be left to astronomers but to generate special monographs for each subject of interest to them.[23] This conclusion followed from his consideration of Newcomb's analysis of the constant of precession.

The most important point of difference between Boss and Newcomb was on the relative value of right ascension and declination observations, and the use of only one star catalogue: Auwers' reduction of Bradley's observations. According to Boss the only way to remove, as much as possible, the errors, both systematic and accidental, was to rely on a number of catalogues, and to make Bradley's observations play a less important role. This, of course, meant a completely new analysis of the star observations, and the redetermination of the proper motions, which led to Boss' second point.

> There is no emergency calling for the immediate adoption of new values of the precessional motion, because there is every reason why there should be a period of increased activity in this line of research. Investigations in stellar problems will probably characterize the astronomy of the twentieth century, as research upon the solar system has been the most remarkable feature in the astronomy of the century now closing.[24]

In a further argument, which rings of Newcomb's earlier criticism of Leverrier, Boss noted that a "rich abundance" of observations on bright, as well as faint, stars already existed for a more comprehensive analysis of precession.

As the controversy went on, Boss saw that it was necessary to present a data analysis, rather than general remarks about how to use the data. In his third paper, Boss discussed each of Newcomb's techniques, he described how the study should have been conducted, and placed particular emphasis on the use of more catalogues. Boss pointed out the wide variation in the corrections to the precessional motion published in the few years since New-

comb's catalogue appeared in 1882. He added that some of these works considered the error effect introduced by the character of telescopic images. Boss argued that, since all observations taken down to 1840 would have had bad images, thereby introducing an added source of error, it was questionable to give Bradley's observations the same weight as Greenwich observations taken between 1857 and 1864. To illustrate how he would approach the problem, Boss gathered information from five catalogues showing a deviation from the equinox position chosen by Newcomb in 1882.[25] Furthermore, Boss did not agree with Newcomb that there were fewer sources of uncertainty in declinations than in right ascensions, and he pointed out a systematic, periodic error in the declination, due to the proper motions in right ascension.

In reply, Newcomb stated that the proper motions of the Bradley stars were known more accurately than those of fainter stars. The use of faint stars was objectionable, because the errors introduced by personal equation of two sets of observers were greater than the precessional effect. The thinking on both sides of this issue rested on qualitative judgments of what observations were needed to obtain a good value of the motion. While Boss argued that more observations were available than Newcomb utilized, he preferred to gather still more observations, and give greater weight to more recent measurements. Newcomb, on the other hand, felt that his methods of analysis had already eliminated the difficulties raised by Boss.

Mixed with Newcomb's outlook on the problem was his desire to see an acceptable value universally adopted, so as to give a consistent base to subsequent catalogues. Then astronomers would not "be under the constant necessity of using provisionally corrected values of the precession." While Boss was prepared to wait for further information, Newcomb could not wait, both because of temperament and age. In spite of Newcomb's protestations that he disclaimed "in strongest manner any desire to force the conclusion that my work ought to be adopted," he thought most highly of his own values, and greatly desired their adoption.

Resolution

The case of the dissidents persuaded some people, because in a poll taken by Harkness, the new Director of the Almanac Office, fourteen favored the new constants and ten opposed.[26] Word of the "revolt in America" reached England; an article appeared in *The Observatory* of December 1897. Downing, fearful of his own position, having taken the initial steps to include the recommendations in the *Nautical Almanac* for 1901, wrote Newcomb for information. In reply, Newcomb predicted that adoption would occur within

two years.[27] But adoption in either country was not to be easy. Strengthened by the American resistance, the Greenwich astronomers began to object. They brought the problem before the Royal Astronomical Society in March 1898, but Downing had already reached the Admiralty with his arguments, and the committee apparently knew it. With the review committee of the Society favoring Downing's arguments, the Greenwich astronomers backed down.[28] (The French and German astronomers appear not to have gone through such an ordeal.) The solution reached in both England and the United States was to provide a transition period for the almanacs, and to use the old constants for the apparent places of stars in appendices. In this way, observatories in the midst of a catalogue, such as Greenwich was, could finish them on the same basis, and switch to the new constants for any new work.[29]

The ephemerides that appeared in 1901 were calculated with the new constants, following the resolution of the Conference of 1896. The constant for solar parallax was the only one adopted outright. Calculations employing both the old and new constants for precession, nutation and aberration appeared in the volumes, with those of the old constants relegated to appendices. While the British chose only Newcomb's value of the mean obliquity, the others used both values. Auwers did not publish his star catalogue until 1904, so the ephemerides adopted Newcomb's new star list, but the American office added corrections to the list.[30]

The tables of the Sun and the planets developed in the American office were adopted universally by 1903. In the 1901 volumes, computers all employed the Almanac Office tables for the Sun, Mercury, Venus, Jupiter and Saturn. However, the Americans were the only ones still using Newcomb's early Uranus and Neptune tables; the Europeans employed the Paris Observatory tables published by Leverrier in 1881. By 1903, Newcomb's table for Mars, and his updated tables for Uranus and Neptune had been published and these also came into general use, replacing Leverrier's. These astronomical constants and tables with occasional modifications remained in general use until the advent of electronic computers and earth satellites in the 1950s. Evidence from satellite observations and computer calculations revealed necessary adjustments in the constants, and these were made and adopted at a conference in 1961.

NOTES

1. Charles Sanders Peirce, *The Evening Post*, January 12, 1901.

2. Simon Newcomb, "Introduction," *Astronomical Papers Prepared for the Use of the American Ephemeris and Nautical Almanac*, I, p. viii.

3. Ibid, p. x.

4. Ibid.

5. "Report of the Secretary of the Navy," 45 Cong., 2 Sess., House Ex. Doc. 1, Series 1799, pp. 119-120.

6. Simon Newcomb to Alexander Agassiz, 5/15/1881, copy in the Simon Newcomb Papers (SNP), Library of Congress.

7. For Newcomb's suggested changes and the Committee's recommendations, see "Appendix II," *American Ephemeris and Nautical Almanac for the Year 1882* (Washington, 1879).

8. "Report of the Secretary of the Navy," 45 Cong., 2 Sess., House Ex. Doc. 1, Series 1799, pp. 119-120; p. 120.

9. The complete list included Greenwich, Palermo, Paris, Konigsberg, Dorpat, Cambridge, Berlin, Oxford, Pulkova, Washington, Leiden, Stras burg and the Cape of Good Hope.

10. Simon Newcomb, "The Elements of the Four Inner Planets and the Fundamental Constants of Astronomy." *Supplement to the American Ephemeris and Nautical Almanac for the Year 1897*, (Washington, 1895).

11. Ibid, p. 1.

12. Simon Newcomb, "Catalogue of 1098 Standard Clock and Zodiacal Stars," *Astronomical Papers Prepared for the Use of the American Ephemeris and Nautical Almanac*, I (1882), 147-314.

13. Lewis Boss, *catalogue of 627 principal standard stars;* . . . (Albany: Dudley Observatory, 1904).

14. SN to Lewis Boss, 2/28/91, Letterpress copy in SNP.

15. Arthur M. W. Downing to SN, 6/22/94, Letters received by the Nautical Almanac Office, General Correspondence, 1885-1897, Records of the Naval Observatory, RG 78, National Archives and Records Service.

16. A. M. W. Downing to SN, 7/18/95, Letters received by the Nautical Almanac Office, General Correspondence, 1885-1897, Records of the Naval Observatory, RG 78, National Archives and Record Service.

17. Gill proposed the conference to Newcomb about April 1, 1894. I have not found Gill's letter, but Newcomb's response to Gill's proposal can be found in SN to DG, 4/23/94, Letterpress copy in SNP.

18. "Report of the Secretary of the Navy," 54 Cong., 1 Sess., House Doc. 3, Series 3379, pp. 114-116.

19. SN to the Secretary of the Navy, 9/26/96, Letters sent by the Nautical Almanac Office, General Correspondence, 1885-1897, Records of the Naval Observatory, RG 78, National Archives and Records Service.

20. Simon Newcomb, "A New Determination of the Precessional Motion," *Astronomical Journal*, 17 (June 11, 1897), 161-167.

21. *Conférence Internationale des Etoiles Fondamentales de 1896, Procès-Verbaux*, (Paris: Gauthier-Villars, 1897).

22. Lewis Boss, "Note on Professor Newcomb's Determination of the Constant of Precession and on the Paris Conference of 1896," *Astronomical Journal*, 18 (August 11, 1897), 9-12; Simon Newcomb, "Reasons for the Adoption of New Values of the Precessional Motion; A Reply to the Remarks of Boss in A. J. 410," *Astronomical Journal*, 18 (September 27, 1897), 33-35; Lewis Boss, "The Paris Conference and the Precessional Motion (Second Paper)," *Astronomical Journal*, 18 (January 4, 1898), 113-118; Simon Newcomb, "Remarks on the Precessional Motion: A Rejoinder," *Astronomical Journal*, 18 (February 2, 1898), 137-139; G. W. Hill, "Observations on Professor Newcomb's Determination of the Principal Element of Precession," *Astronomical Journal*, 18 (February 10, 1898), 153-156; Lewis Boss, "The Precessional Motion and the Paris Conference (Third Paper)," *Astronomical Journal*, 18 (March 19, 1898), 169-176; Simon Newcomb, "Remarks on Prof. Boss's Third Paper on the Precessional Motion," *Astronomical Journal*, 19 (April 14, 1898), 2-3; and Lewis Boss, "Note on the Foregoing Communication," *Astronomical Journal*, 19 (April 14, 1898), 4-5. See also, Seth C. Chandler, "The Aberration-Constant of the French Conference," *Astronomical Journal*, 18 (February 10, 1898), 149-152; in this article, Chandler used essentially the same arguments that Newcomb employed at the conference.

23. Boss, "Note on . . . Precession and . . . Conference," p. 12.

24. Boss, "The Paris Conference (Second Paper)," p. 113.

25. Boss, "The Precessional Motion (Third Paper)," p. 172.

26. Downing learned the results from Harkness and reported them to Newcomb. AMWD to SN, 10/15/97, SNP.

27. A. M. W. Downing to SN, 4/19/98, SNP. I have not found Newcomb's letter with the prediction.

28. A. M. W. Downing to SN, 3/15/98, SNP. For the British controversy, see *The Observatory*, December 1897 through February 1898.

29. See the *American Ephemeris and Nautical Almanac for the Year 1901* and the British *Nautical Almanac for the Year 1901*.

30. For a brief history of efforts to alter the constants, see G. A. Wilkins, "The System of Astronomical Constants. Part I," *Quarterly Journal of the Royal Astronomical Society*, 5 (March, 1964), 23-31.

AN OVERVIEW OF THE U. S. NAVAL OBSERVATORY

Gart Westerhout
Scientific Director

It is my great pleasure to welcome you to the fifth and last part of our 150th anniversary celebration. The other events, for those of you who were not there, consisted of a series of official remarks on Friday morning, a symposium on historical items on Friday afternoon, a feast at which Commander Hammer was carving a pig – I think the head of the pig is still in the refrigerator – and on Saturday a tremendously successful open house, which was attended by a number of people estimated conservatively at 6100.

In this last part we are going to put the past behind us and discuss what we are doing at the U. S. Naval Observatory today and what we are planning to do in the future.

I always like to call to mind John Quincy Adams, who is the person who was really enthusiastic in the 1820s about setting up a national observatory but never got there because he was always doing exactly the opposite of what Congress wanted to do. In fact, it was because of John Quincy Adams that the Observatory is not run by the Smithsonian. When James Smithson gave the big grant to the U. S. Government to do something in science, Congress accepted the grant with thanks but added a footnote to the law saying that this money shall under no circumstances be used to set up a national observatory. Congress did that because John Quincy Adams immediately proposed construction of such an observatory with the money from Smithson. So we would have been the Smithsonian National Observatory instead of the U. S. Naval Observatory if Adams hadn't had so many enemies in Congress.

For those of us who occasionally forget what the U. S. Naval Observatory is all about, let me just repeat the mission statement:

> Make such observations of celestial bodies, natural and artificial, derive and publish such data as will afford to United States Naval vessels and aircraft, as well as to all availing themselves thereof, means for safe navigation, including the provision of accurate time; and while pursuing this primary function, contribute material to the gen

eral advancement of navigation and astronomy.

This statement sounds beautifully old-fashioned except for the words "celestial bodies, natural and artificial," which indicates that this mission was changed somewhere in the late 1950s, with the first artificial satellite. So it is not all that ancient a statement.

For the guests among us, I want just very briefly to put on a slide of our organization (Figure 1), so that you know where the other speakers in this afternoon's program fit. The Superintendent, of course, is in charge of all divisions: Administrative Management Division, Public Works, Supply and Fiscal, Security, and scientific divisions – the Transit Circle Division (Dr. Hughes), Nautical Almanac Office (Dr. Seidelmann) and Time Service Division (Dr. Winkler). The Time Service Substation in Richmond, Florida, is directly under the Time Service Division, and the Flagstaff Station, under Dr. Ables, reports directly to the Superintendent. Finally, the Exploratory Development Staff, under Dr. Routly, reports to the Scientific Director. The dotted lines in Figure 1 mean that I have the scientific coordination and supervision.

Now, to put today's talks in perspective, let me briefly discuss the goals of the Observatory. This will then provide the framework within which the others will be speaking. The Transit Circle Division is in charge of determining the exact positions and motions of the stars, i.e. compiling star cata-

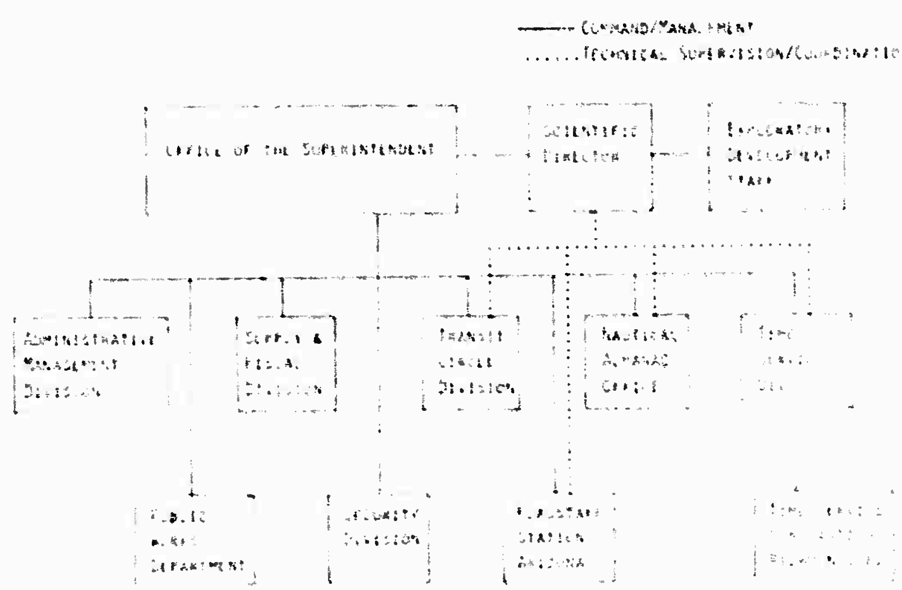

Figure 1. Organization chart of the Naval Observatory, December 1980

logues, and, most importantly, of establishing the fundamental stellar reference frame. The Nautical Almanac Office prepares the almanacs (which includes developing the underlying planetary theories), and is in charge of providing data on all astronomical phenomena for the public. The Time Service Division determines universal time and atomic time, and provides clock synchronization worldwide. The Exploratory Development Staff is in charge of astrographic catalogues as well as double stars, and together with the Flagstaff Station is providing the extensive parallax determinations. Finally, supporting R & D is performed both here and in Flagstaff. Flagstaff is mainly an R & D station.

Since the speakers who follow will be discussing this supporting research in some detail, I would like to concentrate at this point on some of the things that are needed in order to keep up with our mission and to fulfill the mission in the way that it is supposed to be fulfilled, namely, very well. Our needs over the next 20-30 years include: 1) More accurate star positions. Already it is clear that the star positions, as needed by the United States Navy and the Department of Defense, are almost getting to the point that they are not accurate enough. I am speaking here not only of the optical but also of the infrared and ultraviolet ranges. 2) The Earth's position with respect to the stars. I mean here the motion of the Earth in its orbit as well as its rotation and the influence of the other solar system bodies on the Earth and its motion. 3) The area of timing capabilities, including a thorough upgrading of the Master Clock and better clock synchronization worldwide. The clocks around the world that are taken care of by the Time Service Division will be required to be better synchronized with the Master Clock here. 4) Orbital motion predictions, i.e., celestial mechanics leading to the material that goes into the almanacs. All of these needs require basic research as well as exploratory development; new methods and equipment are required to satisfy them. Such research can be done at the Naval Observatory, the Naval Research Laboratory, other Navy and DoD installations, universities and industry. It should be funded by the Navy and DoD.

We have a bit of a problem in the Naval Observatory: we are unique. That is a problem in our general dealings with the Department of Defense and the Government, as well as with our colleagues in the astronomical world. The reason is that we are required to have a dual capability. In the first place, we are definitely required to produce. We have to disseminate data on schedule, whether in the form of almanacs, precise time (required on a second's notice), or star catalogues for which precise positions or predictions of positions are required (on somewhat longer notice). We have to produce astronomical data for a user, quite a different situation than typically

exists in a university environment.

At the same time, in order to provide a quality product, we have to do the forefront R & D that allows us to provide the product with the necessary accuracy. We are the only institution in the United States that determines fundamental stellar positions. We are the only source of Earth rotation data in the United States, although the National Geodetic Survey, which is currently responsible for the determination of polar motion, is planning in the next four years or so to measure, via VLBI, all components of Earth rotation. We are not quite sure yet, however, how this will satisfy the day-to-day DoD users. We own a Master Clock which far outweighs any other in the world. We are solely responsible for improving our own operations, and we have the expertise in many areas. That is to some extent a problem because we have to do both aspects of the work.

Here is another example of why we must continue looking to the future. We are at a juncture. Until ten years ago whatever we produced was always ten or a hundred or maybe a million times better than anybody needed. This held for star positions, for the provision of precise time, and for the observations that were made to assure the quality of the almanacs. Rapid technological development has suddenly changed that whole situation. The requirements that we now get and the expected future demands are such that a quite major effort by the Naval Observatory is in order to meet those future requirements. And, of course, when we ask the various users to do something about it, we always hear this statement: "Why should we pay? You're supposed to provide that according to your mission statement, so why are you asking us for money?" Accuracy improvement in all the areas in which we are working, with whatever means we can reach it, using the most modern means of science and technology, is in my opinion the name of the game over the next ten years.

Let me mention what I consider to be the major projects needed: 1) Upgrading the Master Clock to make its precision at least a factor of ten better. 2) Developing time transfer technology, i. e., getting time to other parts of the globe with an accuracy far surpassing present accuracy. Whether we do that by laser ranging of satellites, Very Long Baseline Interferometry (VLBI), or via the Global Positioning System (GPS) or other satellite systems, is another matter (that is where the research aspect comes in). 3) Improving the fundamental stellar reference frame. Very accurate determination of star positions and motions requires improved transit circle telescopes, other methods of measuring large angles, and studies of the atmosphere. Part of our work will continue to be done from the surface of the Earth, and we had therefore better know the atmosphere. But ultimately, the need for [illegible]

improved fundamental reference frame will perhaps lead to an astrometric satellite, a satellite that can do those measurements that are needed with the greatest accuracy in the same environment in which space vehicles will use this information. 4) And finally, better knowledge is needed of the motions of the planets and the Earth.

With that overview of the needed projects, I will open the floor to the speakers of the afternoon.

1984 AND 2001: THE CURRENT AND FUTURE ACTIVITIES OF THE NAUTICAL ALMANAC OFFICE

P. Kenneth Seidelmann
Director, Nautical Almanac Office

I think we could compare the celebration of the 150th anniversary of the U. S. Naval Observatory to the story of *A Christmas Carol* by Charles Dickens. The events Friday afternoon were an indication of the Observatory's spirits past, the event Friday evening was the consumption of the spirits present, and today we are to have talks by the spirits of the Observatory's future. Before speaking of the future, I'd like to speak just briefly about the beginning of the Nautical Almanac Office, which really was not 150 years ago.

The Nautical Almanac Office did not start until 1849 — we had some comments last Friday concerning the reason why the Nautical Almanac Office was separate from the Naval Observatory. Actually, the Nautical Almanac Office joined the U. S. Naval Observatory here in Washington about 1893. From Table 1 you can see that not only were we slow starting with respect to the Naval Observatory, we were also slow from an international point of view. The French have been producing almanacs for 300 years, the British for over 200 years, and we are the fifth entry with 125 years. The first three countries are now using our publication in a cooperative preparation and publication effort. This also indicates the international character of the work we do, preparing publications that most of the countries of the world either use, imitate or duplicate.

Table 2 is a list of the publications that we are producing. The new title

Table 1. *Initial Publication of National Ephemerides*

Connaissance des Temps (France)	1679
The Nautical Almanac and Astronomical Ephemeris (Great Britain)	1767
Berliner Astronomisches Jahrbuch	1776
Efemerides Astronomicas (Spain)	1791
The American Ephemeris and Nautical Almanac (United States)	1855
Annuaire Astronomique (U.S.S.R.)	1923
Japanese Ephemeris	19[illegible]
Indian Ephemeris and Nautical Almanac	1958

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Table 2. *Series Published by the Nautical Almanac Office*

The Astronomical Almanac
The Nautical Almanac
The Air Almanac
Astronomical Phenomena
Almanac for Computers
Planetary and Lunar Coordinates
Sight Reduction Tables
The Ephemeris
U. S. Naval Observatory Circulars
Astronomical Papers of the American Ephemeris
Publications of the U. S. Naval Observatory

for *The American Ephemeris and Nautical Almanac* and *The Astronomical Ephemeris* is *The Astronomical Almanac*, beginning with the edition for 1981. I also wish to point out the publication *Planetary and Lunar Coordinates*, which provides ephemerides for the Sun, Moon and planets at reduced accuracy and further in advance. The current edition of this publication is for 1980-1984, the next edition will be for 1984-2000. The publications are produced within the office, with the principal responsibility borne by Paul Janiczek and LeRoy Doggett.

In addition to the publications, we also produce, to the extent possible, whatever data people want in the form that they want it. This includes machine-readable data, computer printouts, printed tables, legal certifications, letters, and predictions of eclipses and occultations. This effort is basically by Sol Elvove, Jean Dudley, Marie Lukac, Ken Pulkkinen, Alan Fiala, Tom Van Flandern and others. Our publications and data are produced not only for the Navy, the Defense Department, and other government agencies, but also for the scientific community and the general public.

Similarly, our research benefits not only the government, the Defense Department and the Navy, but also the scientific community. In Table 3, current and future accuracy requirements are specified. It is future improvement that we must address.

One of our research efforts is the preparation of the fundamental ephemerides. Various comments can be made about the fact that the new ephemerides are to be introduced for 1984, the year made famous by George Orwell. A recent article[1] has graphically displayed the history of the determinations of the mass of Pluto (Figure 1), and concluded that in 1984 the mass of Pluto will become negative. It is this kind of application of one's scientific work that flatters a person.

We are preparing new fundamental ephemerides because the current

Table 3. *Accuracy Requirements for Ephemerides*

Navigational Almanacs	±3"
Astronomical Requirements	
Instrument Setting	±1"
Theoretical Investigation	±0."1
Reference System	
Currently	±0."01
Future Anticipated	±0."001
Space Missions	
Best Accuracy Possible	
Realistic Knowledge of Accuracy	
Tradeoff of Fuel versus Accuracy	

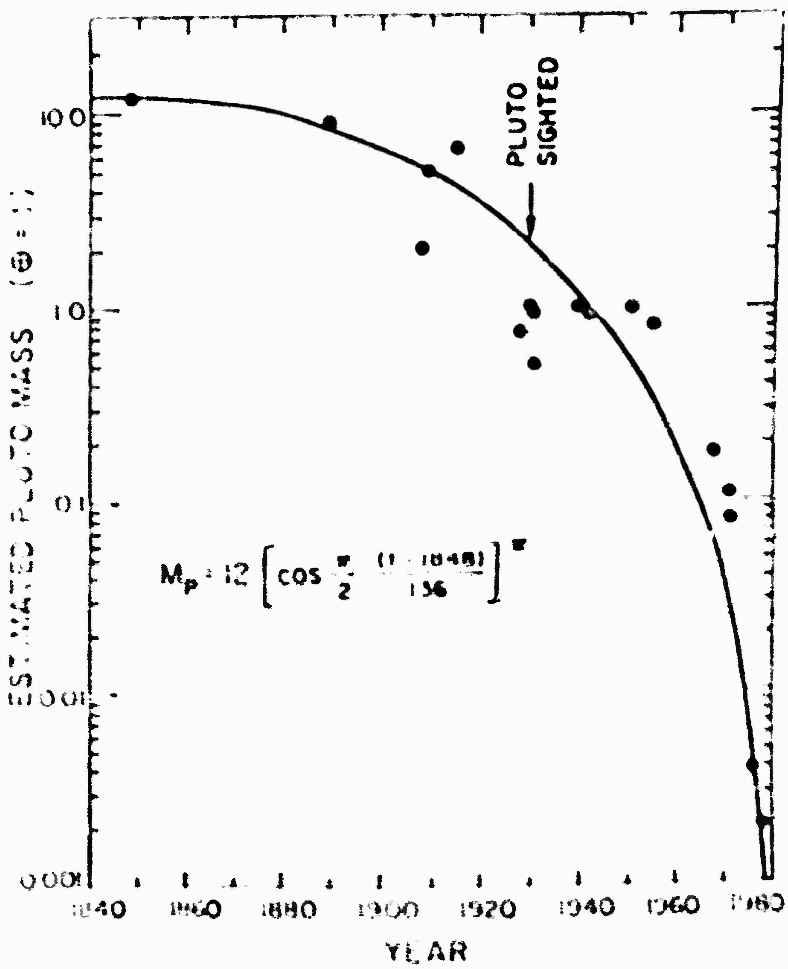


Fig. 1. Mass determinations of Pluto

theories lack the accuracy required. The improved accuracy affects the positions of the Moon and planets; it affects how the planetary ephemerides are used to determine the lunar ephemerides; it affects the tidal calculations, the libration of the moon, the location of the equinox, and our understanding of the solar system.

For this purpose, we began a program to introduce new astronomical constants. After Dr. Norberg's talk, which recounted the problems Newcomb had in introducing astronomical constants, one person commented to me, "You're not having it so bad, Ken!" We are having some problems, and we are in a different environment. The determination of astronomical data and constants is much more rapid today than it was in the 1890's. One Voyager mission going past a planet learns a lot, and it becomes known quickly and accurately.

One of the improvements needed is the determination and introduction of a new theory of nutation. In this effort there is cooperation within the Naval Observatory. The heavy line in Figure 2 indicates a comparison of the new nutation theory with the current nutation theory, which is on the straight line; the boxes indicate the nutation values determined from the data being obtained from the radio interferometer at Green Bank. This plot is by George Kaplan.

We need an equinox for the FK5, an origin for the coordinate system, and for the observational period we need values for ΔT, the difference between ephemeris time and universal time. Figure 3 gives a plot of ΔT for 1820–1980; the crosses are Brouwer's values and the circles are a recent analysis by Tom Van Flandern and Marie Lukac. Additionally, we need to develop new general theories, which is an effort by Peter Espenschied and Tom Van Flandern; to perform numerical integrations, which is the work of George Kaplan; to determine partials, which is Ken Pulkkinen's activity; and to collect all the observational data and compare it with these ephemerides, which is the effort of Ernie Santoro, with the help of Ed Jackson, of the Transit Circle Division.

To show what this means, Figure 4 is a plot for Pluto of the differences in right ascension between the observations and the ephemeris that we are currently publishing in *The Astronomical Almanac*. I will grant that Pluto is the worst case, and Pluto will continue to be a problem due to the short observational history. Figure 5 is a comparison between the Pluto observations and a new ephemeris in right ascension. You can see that a significant improvement has taken place. We can also compare the declination residuals of Pluto as currently published (Figure 6) and the residuals from a new ephemeris (Figure 7). I will return to the question of why Figure 7 does not

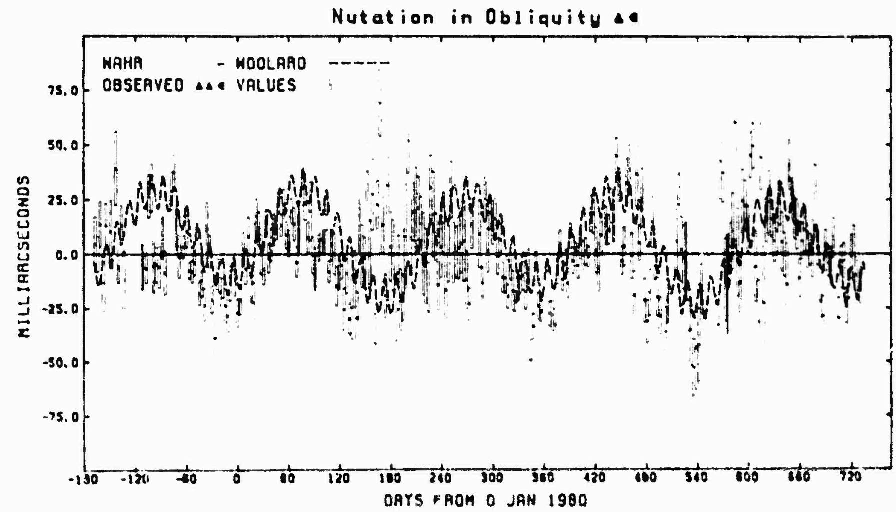


Fig. 2. Difference between new and current (pre-1984) nutation theories (dashed line), and difference between observations and current nutation theory (boxes).

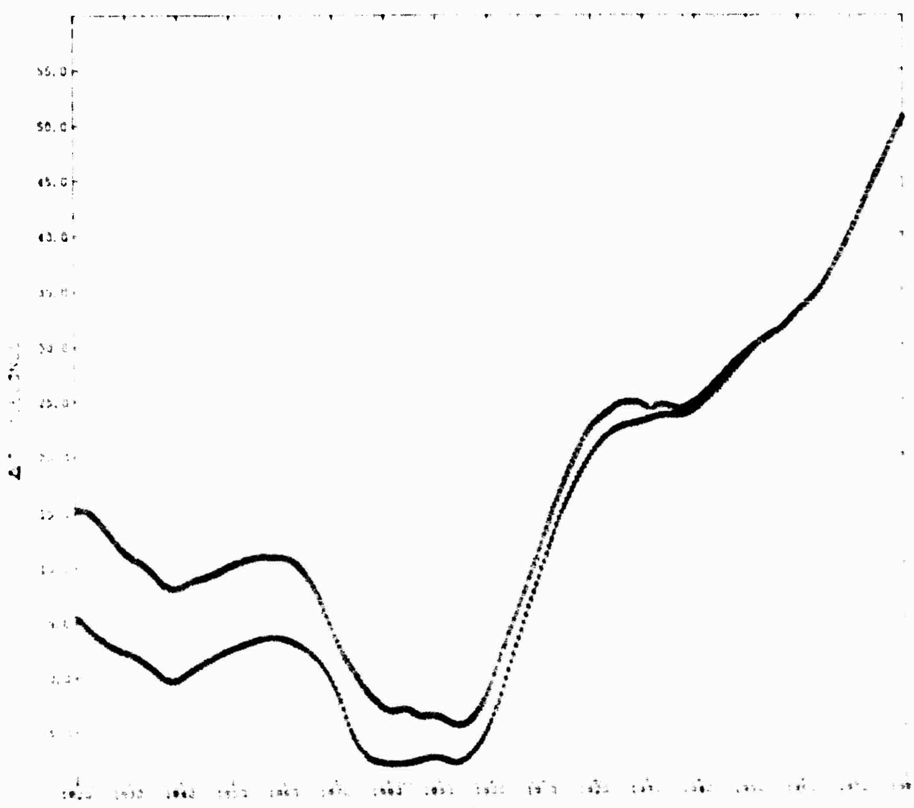


Fig. 3. Differences between ephemeris time and universal time, 1820-1980, according to Brouwer (crosses) and Van Flandern and Lukac (circles).

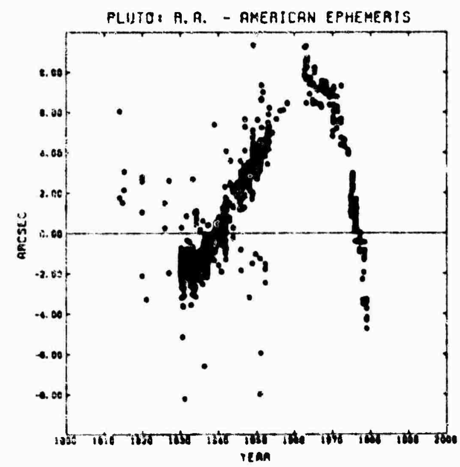


Fig. 4. Pluto: differences in right ascension between observations and *American Ephemeris.*

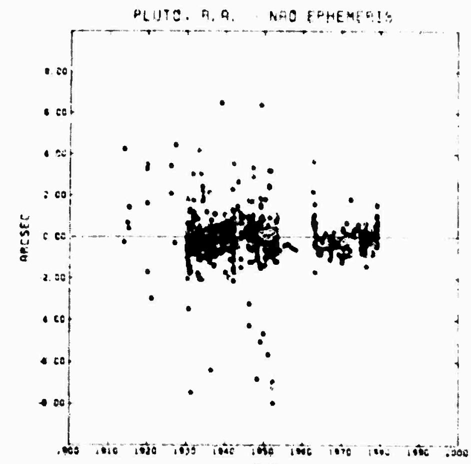


Fig. 5. Pluto: differences in right ascension between observations and improved ephemeris.

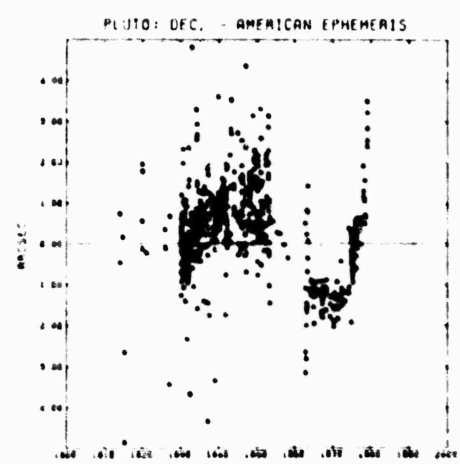


Fig. 6. Pluto: differences in declination between observations and *American Ephemeris.*

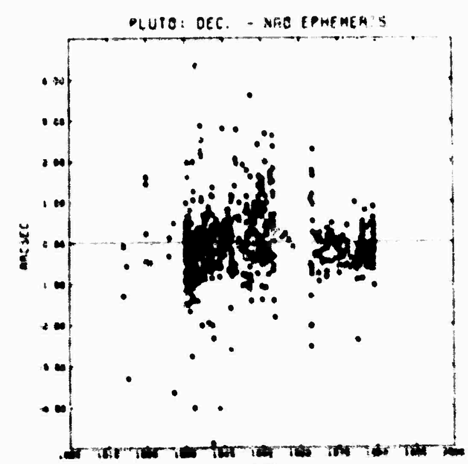


Fig. 7. Pluto: differences in declination between observations and improved ephemeris.

look as good as it might and whether there are systematic differences in that plot.

Let me take another representative planet, Saturn, and compare the observational data in right ascension to the published ephemeris (Figure 8) and to a research ephemeris (Figure 9). There is no systematic difference here, but then we look at the declination observations compared to the current publication ephemeris (Figure 10) and the improved ephemeris (Figure 11). Here we can see the improvements take place. There is a need to look at the systematic effects that are left from these comparisons. This is a glance at the effort to determine improved fundamental ephemerides for the planets.

Let me go on to other research activities. Figure 12 shows the observational equipment set up in Russia for Alan Fiala to observe a solar eclipse. There is additional confidence in your predictions after you have checked them. These observations also provide a unique way of tying together the solar and lunar ephemerides. During a solar eclipse, the two ephemerides must agree with each other. There have also been reports that the size of the Sun is changing. From observations of eclipses obtained today and from observations obtained in the distant past, we can tie down, we hope, the rate of change of the size of the Sun. As shown in Figure 12, a telescope is used to project the image onto a screen and the image is videotaped. We are looking for the precise time of contact, pa[illegible] the contacts at the edge of the path. The observations are made at the edge of the path so that the actual times of events caused by the lunar limb can be observed.

We are also interested in the planetary satellites, so we have an observing program which is the effort of Dan Pascu. Figure 13 shows Mars as photographed with a filter, so that the two satellites of Mars appear against a star background. The same type of observations are made for Jupiter. Figure 14 shows six images of Jupiter with its satellites, and Figure 15 is a photograph of Saturn and its satellites. This effort has been in cooperation with NASA to improve the satellite ephemerides as required for the Voyager missions. To remind you of those missions I'd like to show a now famous picture (Figure 16) of the rings of Saturn, with the massive amount of structure that is present in these rings.

We are also involved with the Space Telescope Widefield/Planetary Camera Investigation Definition Team.[2] This instrument will use a charge coupled device (CCD) as a detector. A ground based camera is now available for the team's learning and testing. We have made observations with this camera to learn how to observe with the device that will be on the Space Telescope, to test the astrometric capabilities of the CCD, and also to do some science

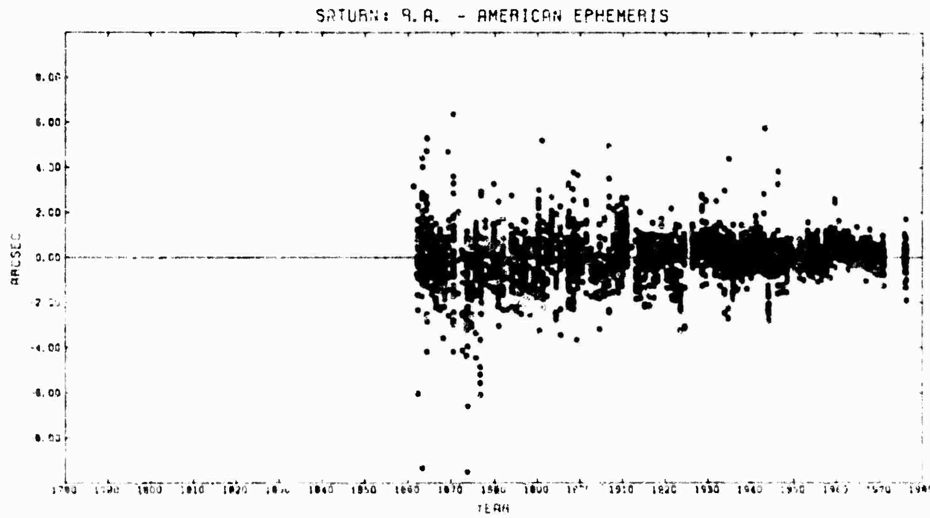


Fig. 8. Saturn: differences in right ascension between observations and *American Ephemeris*.

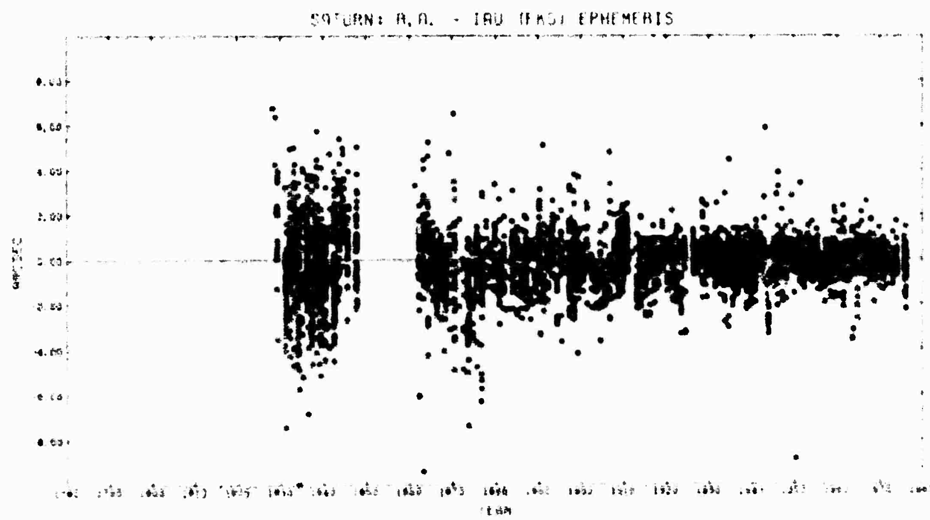


Fig. 9. Saturn: differences in right ascension between observations and improved ephemeris.

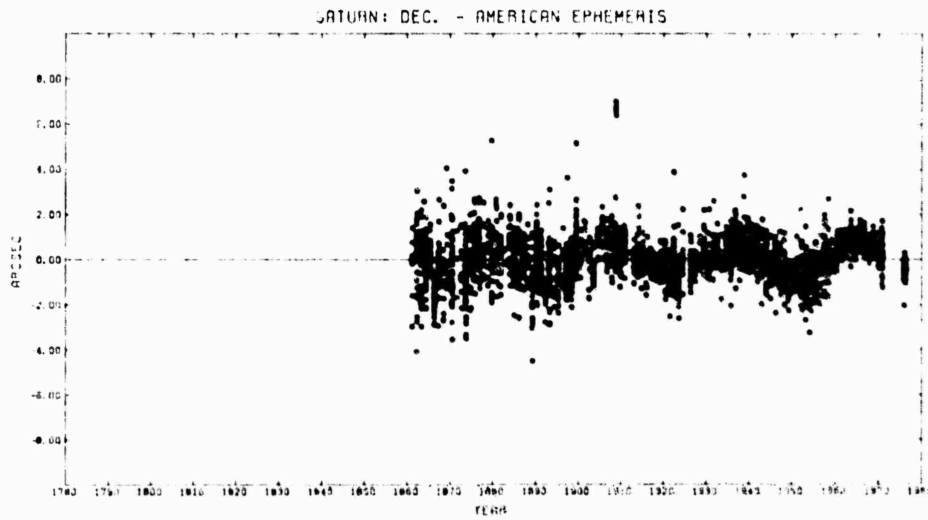


Fig. 10. Saturn: differences in declination between observations and *American Ephemeris.*

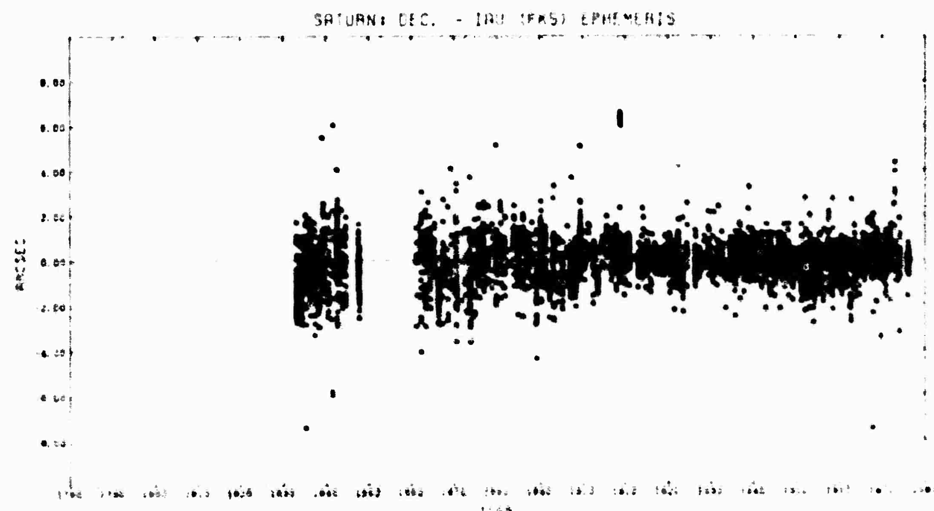


Fig. 11. Saturn: differences in declination between observations and improved ephemeris.

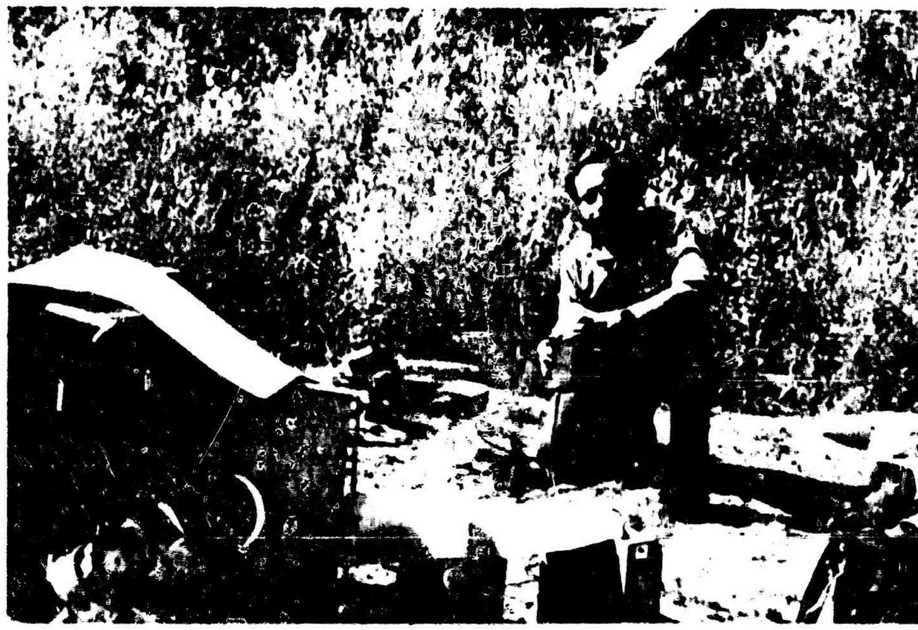


Fig. 12. Alan Fiala making observations of the solar eclipse in Russia, 31 July 1981.

in the process. The top image of Figure 17 shows Saturn with everything we know about Saturn blocked out; Saturn and the A, B and C rings are behind this mask, and we have masked off the satellites of Saturn. This was taken in March 1980 at the Flagstaff Station of the U. S. Naval Observatory at the time of the ring plane crossing. The middle image shows the same frame with the scattered light of Saturn removed. The image of 1980S25 now becomes evident in the west side of the E ring.[3] The bottom image has been stretched so that more of the E ring is evident, and Saturn and the A, B and C rings have been superimposed from another image. Distance scales with units of Saturn's radius, and the orbits of the known satellites are indicated across the bottom.[4]

After proper transformation of six different exposures, we find the distribution of the E-ring material shown in Figure 18.[4] The unexpected conclusion is that the material has a maximum density at the orbital distance of Enceladus. That raises the question of why there is a stable ring of material at the same distance as the satellites. There is no apparent change in the distribution of E-ring material with any of the satellites; they seem to be able to coexist.

I have talked about the fundamental ephemerides, the eclipse observations, the satellite observations and the CCD observing for the Space Telescope. We also have a research effort using the Very Large Array in New Mexico, based on the idea that we might observe minor planets at radio

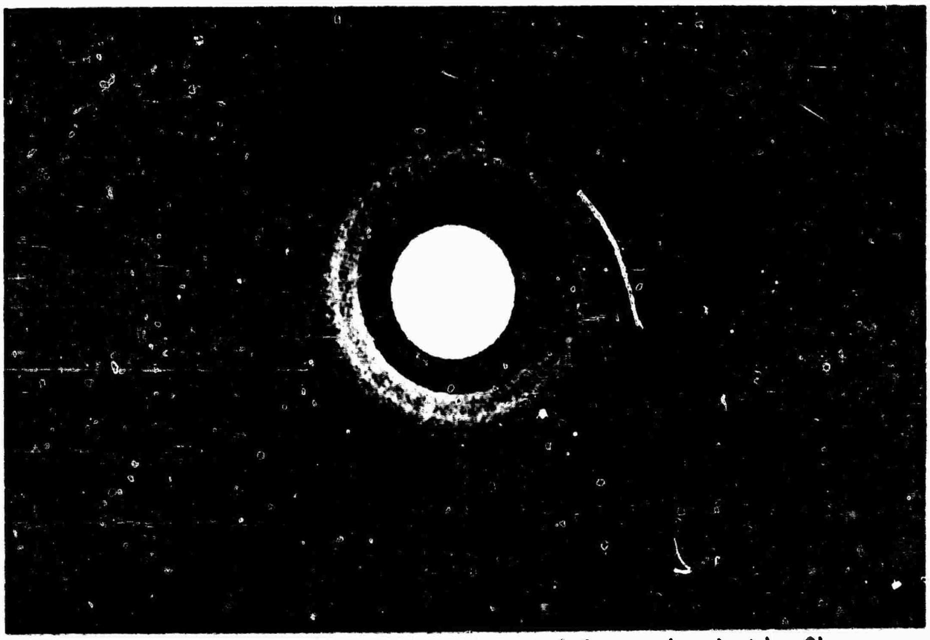


Fig. 13. Photograph of Mars with brightness of planet reduced with a filter to emphasize satellites.

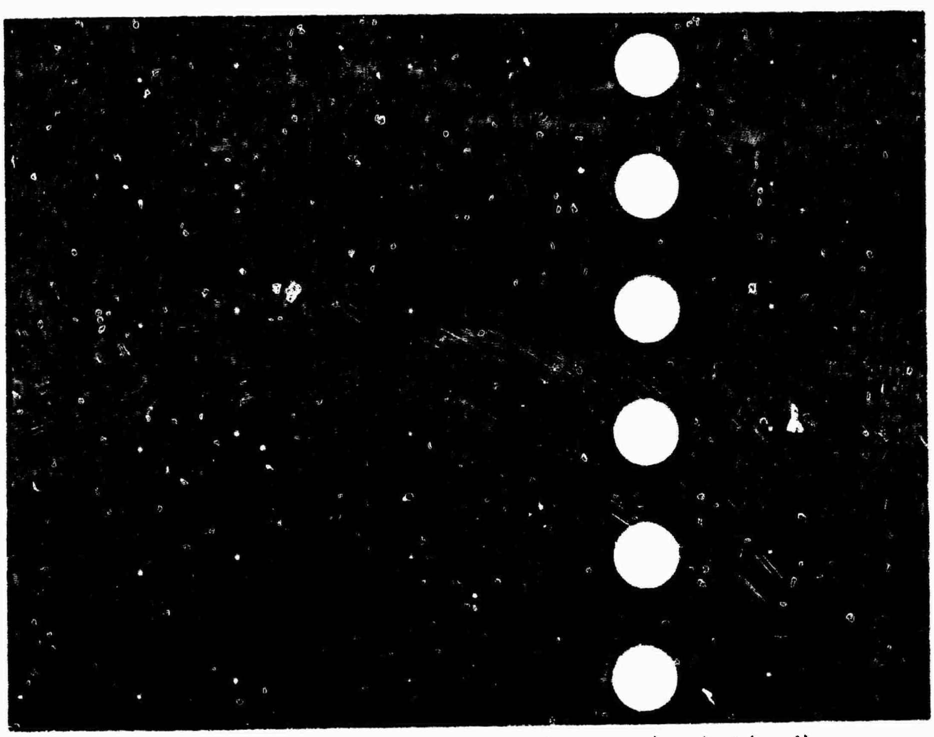


Fig. 14. Images of Juipter with brightness of planet reduced with a filter to emphasize satellites.

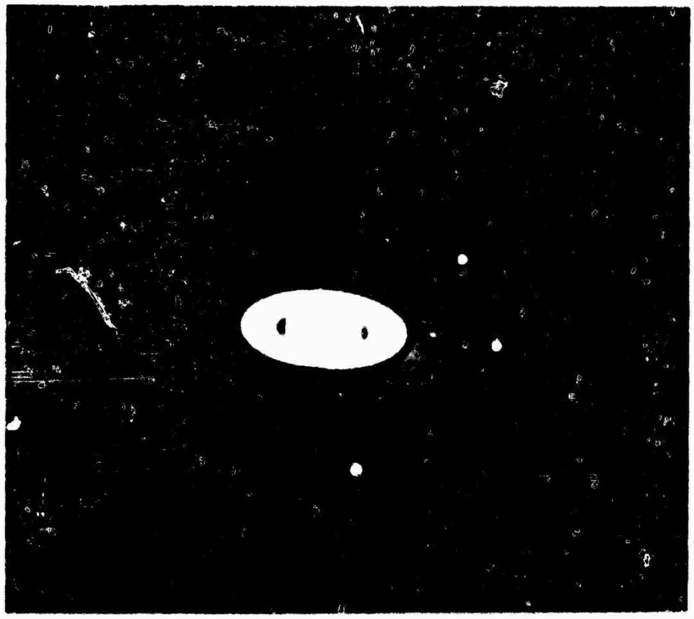


Fig. 15. Photograph of Saturn with brightness of planet reduced with a filter to emphasize satellites.

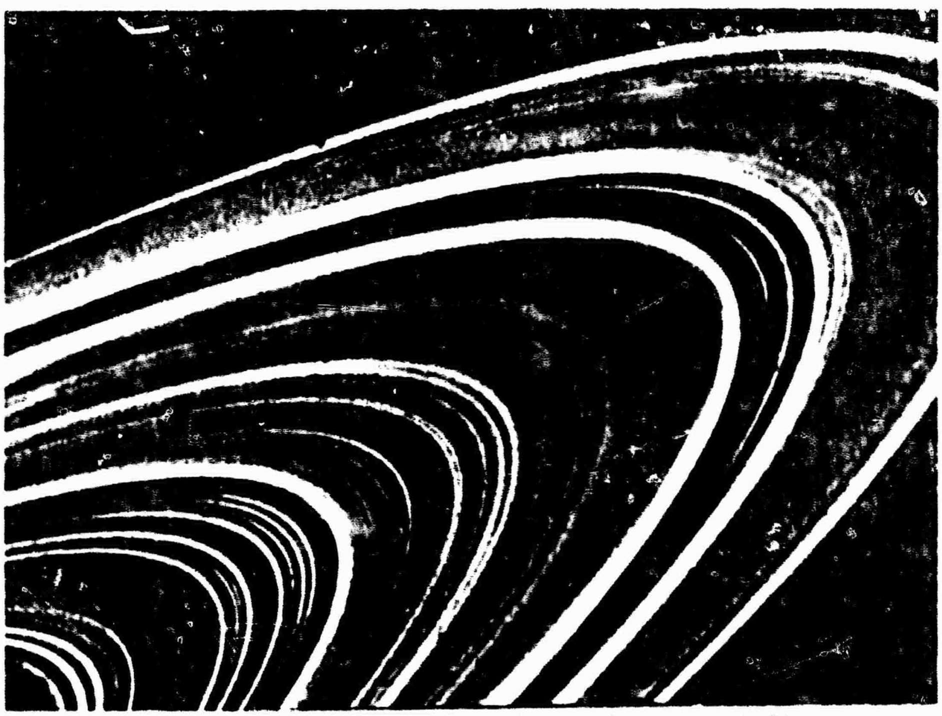


Fig. 16. Rings of Saturn as seen by Voyager I, November 1980.

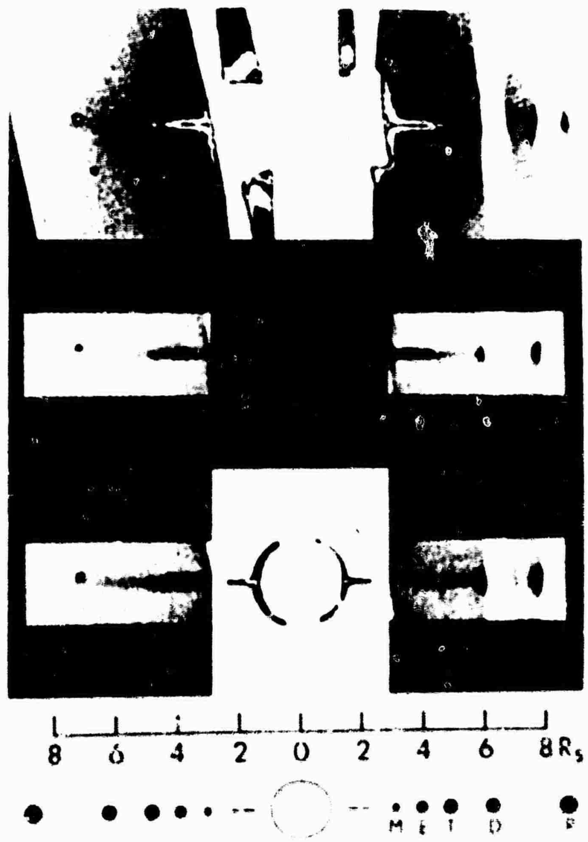


Fig. 17. Images of Saturn with the planet and known satellites blocked out to show the E-ring. The newly discovered satellite 1980S25 appears in the middle frame on the west side of the E-ring. Photo taken with a CCD camera on the 61-inch reflector at Flagstaff in March 1980.

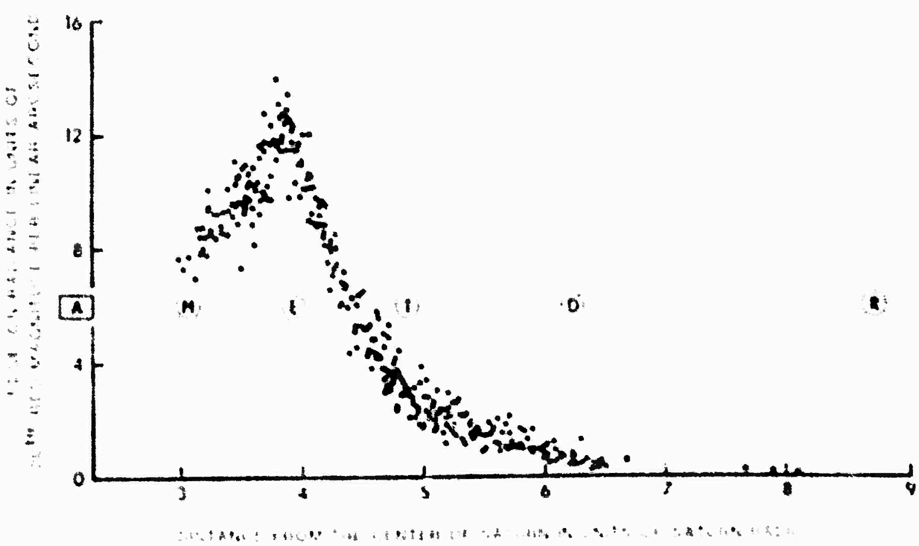


Fig. 18. Distribution of E ring material around Saturn. Locations of satellites are indicated by their first letters. Maximum density is at orbital distance of Enceladus (E).

frequencies. If this is possible, then we ought to be able to tie together the radio or quasar coordinate system with the optical coordinate system, since a minor planet would be a point source in both cases. We also would be able to observe minor planets all the way around their orbits and accurately determine the minor planet orbit and the Earth's orbit. This activity is by Sally Bensusen, George Kaplan and myself, in cooperation with K. J. Johnston of the Naval Research Laboratory and C. M. Wade of the National Radio Astronomy Observatory.

Additionally, we are looking at how we can improve the instrumentation and techniques of navigation. One of the items that has come from this effort is the *Almanac for Computers*, which was fathered and is continued by LeRoy Doggett. This volume has found acceptance among individuals with hand calculators and computers. We are also investigating the feasibility of a day/night digital sextant with a built-in calculator. This is a cooperative effort with S. Feldman of the Naval Surface Weapons Center.

These are current research activities. But, lest I give you the impression that all is well and all problems will be solved by 1984, I'd like to present a list of questions and problems—items that might be solved by the next century or 2001. One of the basic things that we use is a coordinate system with an origin at the equinox, referred to the ecliptic plane. These are concepts that are familiar to all of us. The only problem is, where are the equinox and ecliptic? Do we really know what they are, both in definition and in determining their location?

I mentioned the problem of systematic effects remaining in the residuals. Figure 19 is a plot of the declination of Neptune observations compared to the ephemeris. With a little imagination, but not much, you can see that there is some sort of a systematic effect present in these residuals. If you cannot see it there, maybe you can see it better in Figure 20 with the Uranus declinations. Again, some sort of a systematic effect appears to be present.

Then there is a perplexing problem of Pluto. Table 4 is a comparison of the predictions for Pluto and the current knowledge about Pluto. Most of the predictions were reasonably good; the longitude at the time of discovery being less than six degrees from that predicted. The only problem is that the predictions of Pluto were based on a mass of Pluto somewhere between two and six times the mass of the Earth. We now know that the mass of Pluto is in the neighborhood of 0.002 the mass of the Earth, so the predictions could not possibly be valid. So Pluto was discovered due to the quality of the search. But what was the basis for the predictions?

Let's consider some other questions. What is the cause of the observation differences between the 1800s and 1900s? There seems to be a systematic

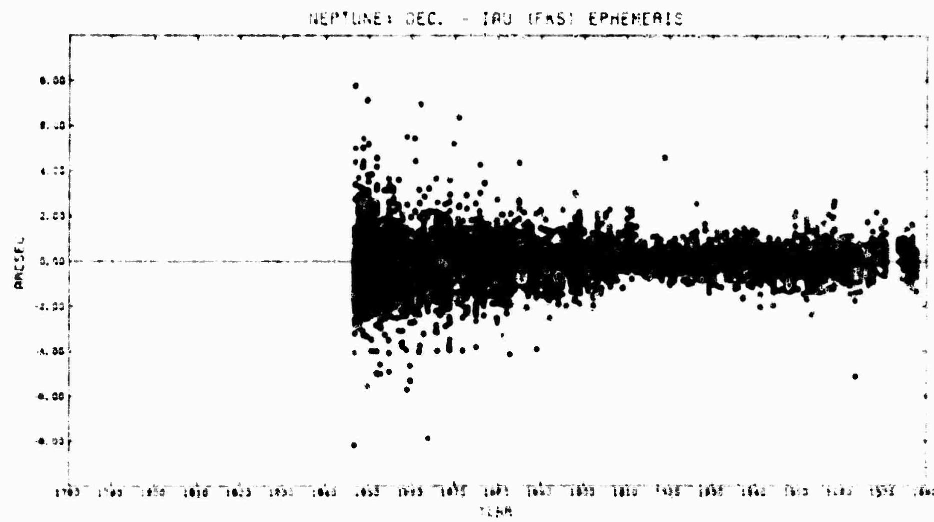


Fig. 19. Neptune: differences in declination between observations and improved ephemeris.

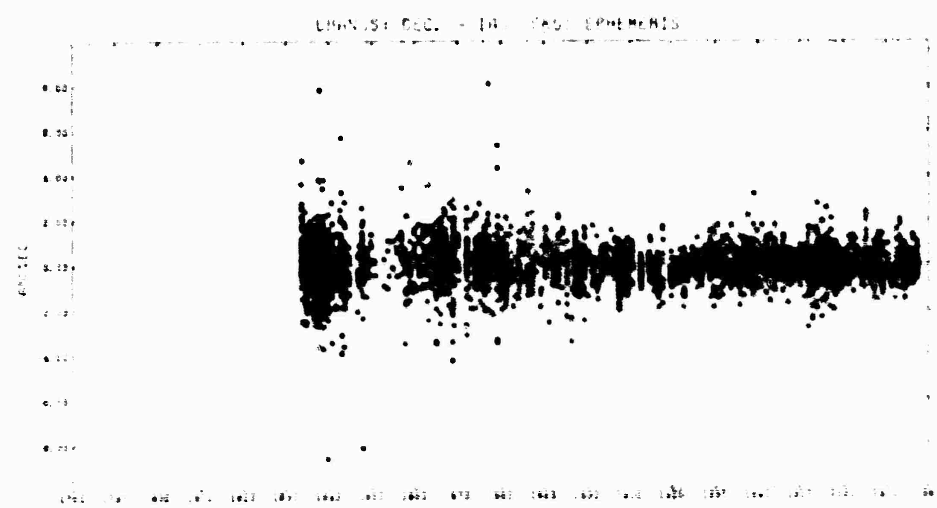


Fig. 20. Uranus: differences in declination between observations and improved ephemeris, showing possible systematic effect.

Table 4. *Pluto: Predicted and Actual Data*

	P. Lowell (1915)	W. H. Pickering (1919)	Actual
Mean Distance (au)	43.0	55.1	39.5
Period (years)	282	409.1	248
Orbital Eccentricity	0.202	0.31	0.246
Longitude of Perihelion	204°.9	280°1	222°.9
Date of Perihelion Passage	1991.2	1720.0	1989.9
Longitude of the Node		100°	109°.6
Orbital Inclination	10°	15°	17°.1
Longitude at 1930.0	102°.7	102°.6	108°.5
Magnitude	12-13	15	15
Mass (Earth = 1)	6.7	2	0.002

difference between observations of these time periods. Is that what is causing the problems of the outer planets, or is it an indication of other problems? Why are there secular discrepancies in the observations of lunar occultations? Or, is the constant of gravity constant? Tom Van Flandern will give you his answer, but not everyone is convinced. Is the solar system stable? Why do we see changes today that should not be continuing any longer? Why aren't the calculated planetary theories as accurate as they were designed to be? Can you explain the dynamics of the satellites of Saturn and the ring system of Saturn? Bob Harrington of the Exploratory Development Staff is working on the satellite question now, and he has some less than flattering comments about my intuition in celestial mechanics. I can only say that I have a lot of company until we understand the dynamics of the co-orbiting satellites. Perhaps by the twenty-first century, we'll know the answers to some of these questions. I hope so.

In conclusion, let me remind you that our job is not just to report the observational data and fit ephemerides to that data. We are required to publish almanacs in advance. This means that we have to predict the positions of the Sun, Moon, planets and satellites at least five years in advance. The real problem is predicting where these objects are going to be, and that requires that we know something about the solar system: its composition, the rings around the planets, what satellites exist, whether there are minor planet satellites or even unknown planets. We have to be able to represent the motion by equations; therefore, we need an accurate observational history with which to compare our calculations and to give us some confidence that we know what we're predicting.

G. Westerhout (Scientific Director, USNO): Thank you very much, Dr. Seidelmann. I am sure there are some people who want to ask questions.

C. A. Ailey (U. of Maryland): What is the current thinking about the braided ring of Saturn?

Seidelmann: The best knowledge that I have is that Goldreich and Tremaine have looked at it, done an order of magnitude calculation, and said that the satellites on either side of the rings could cause what is being observed. That is not to say that the satellites are the explanation, but that from an order of magnitude point of view, it is possible. On the other hand, can you explain all the rest of the gaps and how the source of those gaps relate to the braiding?

G. M. R. Winkler (Director, Time Service, USNO): You show the comparison between your standard ephemeris and a research ephemeris. Is the difference between these two mathematical or did you take up additional physical terms?

Seidelmann: One of the principal differences is the fact that we have more recent calculations than the published ephemeris, so that we have more observational data to take into account. There are also some changes in masses and constants.

N. G. Roman: There has been some discussion recently about prediscovery observations of Neptune by Galileo. Are those of an accuracy that is useful, or of what importance are they?

Seidelmann: Galileo's observations seem to be useful, because they can pin down the position of Neptune to some level of accuracy. The question of what accuracy is a point of argument, but certainly we are going to use these observations as a test for our Neptune ephemeris to see whether we can fit them within their estimated accuracy. The same problem is presented by Lalande's Neptune observations, and that work is currently underway.

R. E. Keating (Time Service): How many comparisons exist for the inner planets with relativistic corrections to the motion of the perihelion and such things? Do these fit the theories well?

Seidelmann: The motion of perihelion seems to fit well, but we have some sort of problem in longitude that we don't understand right now. We can't even tell you where it is coming from. There is a discrepancy of about a

second of arc per century, and this is related to the location of the equinox, the constant of precession, and how accurate transit observations really are. (Laughter.) Speaking first, I can raise such questions, but a rebuttal will come later. Really, the questions become how accurately can they observe the Sun and what sort of systematic effects might be present. They will tell you the problem is our equations of motion, obviously.

NOTES

1. Dessler, A. J. and Russel, C. T., "From the Ridiculous to the Sublime: The Pending Disappearance of Pluto," *E. O. S.*, 61 (Oct 28, 1980): 44.

2. The WF/PC Team is J. A. Westphal, Principal Investigator, J. E. Gunn, Deputy PI, A. D. Code, G. E. Danielson, T. Kelsall, D. G. Currie, C. R. Lynds, B. A. Smith, J. Kristian, W. Baum and P. K. Seidelmann.

3. Seidelmann, P. K., Harrington, R. S., Pascu, D., Baum, W. A., Currie, D. G., Westphal, J. A., and Danielson, G. E., "Saturn Satellite Observations and Orbits from the 1980 Ring Plane Crossing," *Icarus*, 47 (1981): 282-287.

4. Baum, W. A., Kreidel, T. J. N., Westphal, J. A., Danielson, G. E., Seidelmann, P. K., Pascu, D., and Currie, D. G., "Saturn's Ring. I. CCD Observations of March 1980," *Icarus*, 47 (1981): 84-96.

DEVELOPMENTS IN THE TIME SERVICE DIVISION

Gernot M. R. Winkler
Time Service Division

Dr. Seidelmann has characterized the name of his game as prediction. In contrast, the game of the Time Service Division is to know what time it is now and to let our users know that information as soon as possible. Therefore, as I see it, our main concern now and for the next decade is data communications, remote collection of data, processing of these data more extensively than ever before, and dissemination by digital means in near real time.

That, of course, is a vast change from the beginnings of time service, which go back much further than the national ephemerides. The first time service I know of has been described in the famous volume of the Sitzungsberichte of the Prussian Academy of Sciences in 1919, the volume in which Einstein's papers on general relativity appeared. There you find an interesting description of the so-called night clock of Plato's Academy, a very complicated clepsydra which at four o'clock in the morning was to wake up the students so that they were ready to listen to the master. I think that they were ahead of us in that respect because we start later

Aside from this ingenious device there was not much development in time service until 1317, when the French King Charles V completed a beautiful huge tower clock in the Palais Royal in Paris, which is still there. It was made of a weight of 500 pounds that moved slowly through a height of 32 feet in a day. Of course, the whole art of clock making is right there with the question of how you make that movement uniform. Time service really began with this device in 1317 when the King, very impatient with the fact that all the bells in Paris would ring at irregular intervals, issued an edict that henceforth all the church bells would ring the hour and quarter hour in consonance with the Palais Royal. That was the first Department of Defense instruction, if you will, or at least an antecedent of our pertinent DoD instruction. The episode reminds us also that timekeeping and the use of time is closely linked to social activity.

A number of concepts must be distinguished that are constantly troublesome. The first one is that we deal with a variety of "times". The concept of time as an ordering parameter and that is essentially what it is did not orig

inate until medieval times. But that ordering parameter, which today is best represented by clock time as a uniform measure of the progress of processes, must be differentiated from the "time of day", which is a measure of the rotational position of the Earth. Until 1972 the values for these two kinds of time were much closer together than today. Previously, they were kept together by daily adjustments. I still remember in 1956, when I visited the Naval Observatory for the first time, how that daily adjustment was performed. At noon, or early afternoon, it was known how large the difference was between clock time and the time as determined during the previous night's observations, and an adjustment was made quickly in the Master Clock. That change was reflected in changed monitor results of the time signals, and eventually the distributed time signals were adjusted.

All of that has long since disappeared. Since 1960 we no longer adjust clocks every day. Instead, since 1972 about once a year we apply the leap second, a step adjustment now in use in order to provide a more convenient time scheme for the majority of precise time users, who are not interested in universal time or time of day, but in uniform clock time. Many users, however, still need to know the differences between these two, and that means you have to make optical and radio observations of the time of day. Additional data, such as polar motion and other corrections, also have to be determined, and the result has to be disseminated somehow. This leads to the Time Service Bulletins that we issue every week and to the predictions that involve us with statistics and statistical estimation theory. Time service is inseparable from statistical estimation theory because the operation of a clock is a random process--the clock errors are random. One can go one step further and say that with systems being sharply defined, it would be impossible to measure time. An atom, for instance, with entirely sharply defined states, would not have state functions as a function of time. So statistics and timekeeping are inseparably connected.

Another problem in time service is with synchronization. If one measures distance with time or time over a distance, one must become involved with relativity theory. That is purely a scientific matter. But there are also organizational problems. Moreover, we cannot remain completely independent of other countries. Fifty years ago people worried that time signals would be transmitted on different seconds in different countries. Now we are worried whether we are on the same microsecond, because we are coming into an era when large communications and navigation systems have to be interfaced and the question is, "What are the time references in these different countries doing?" That coordination could create many problems.

Some Applications of Time

Now we must say a few words about the applications of time for communications, navigation and science. The applications of time for electronic systems are increasing with the use of more sophisticated concepts of technology.

In communications today we have time requirements down to about five microseconds. Those requirements come about because people want to do the same things simultaneously at two distant sites and they want to continue doing it without the need to interchange many synchronization signals. In the field of secure communications systems, which deliberately scramble the frequencies or the phases, initial synchronization is essential. That synchronization window is at the moment the origin of the requirement for the largest number of clocks in the Navy and the Department of Defense. These secure systems require atomic clocks on each platform and that use is going to increase. Today we deal with about 2,000 cesium beam clocks that are looking at our master clock, and about 10,000 rubidium standards. I expect that number to increase possibly by a factor of ten during the next ten years.

Electronic navigation is an entirely different area of concern. Here we make measurements of the time of arrival of a wave front from either a natural or an artificial source. Although it is true that most conventional electronic navigation is concerned with relative measurements, the absolute method brings such advantages that it is going to increase in importance.

A completely different area again is metrology. The measurement of time is "counting" by nature. It is directly related to the digital process, and as digital technology is exploding, the use of time or frequency as an intermediate measurement parameter, into which one converts those things that one wants to measure with high precision, is a very natural and ongoing application. For example, distance can be measured as a time interval. We can use frequency converters and special transducers for other measurements. That is the reason why calibration laboratories in increasing numbers require more and more accurate time.

Finally, one must not forget that the most precise and demanding applications of precise time are in astronomy. Pulsar researchers and Very Long Baseline Interferometry (VLBI) are users as well as providers of precise time. That is a principle that is true in many areas of timing activity: users can be providers and vice versa, and in time the two become almost inseparable. This has led during the last ten years to a growing community of precise time users who can help one another. By knowing what time it is at your neighbor's house where there is a good clock, if you lose time you have a very easy way to get back into operation. That is true for many electronic sys-

tems today. That is one of the reasons that we find ourselves more and more involved with data communications, because there must be someone who is coordinating these things and who can act as a central depository of data, and who makes the data available in near real time.

In all timing applications propagation delay of the time signal is a major problem. In fact, it may be *the* problem. It is not only a matter of worrying about the geometric delay; in the case of a fixed station on Earth, its determination is a one-time affair. When you talk about timing for satellites or satellite systems, the question of their position is intrinsic to the problem of the measurement of their clocks, or the utilization of their clocks. The same thing is true for the delays through the troposphere and the ionosphere, where we face exactly the same problems as the radio astronomers who have to be able to account for these delays in order to extract the greatest benefits from the observations.

Figure 1 shows one of our portable atomic clocks, which I show here because it will continue to act as a last resource when we need an overall cali-

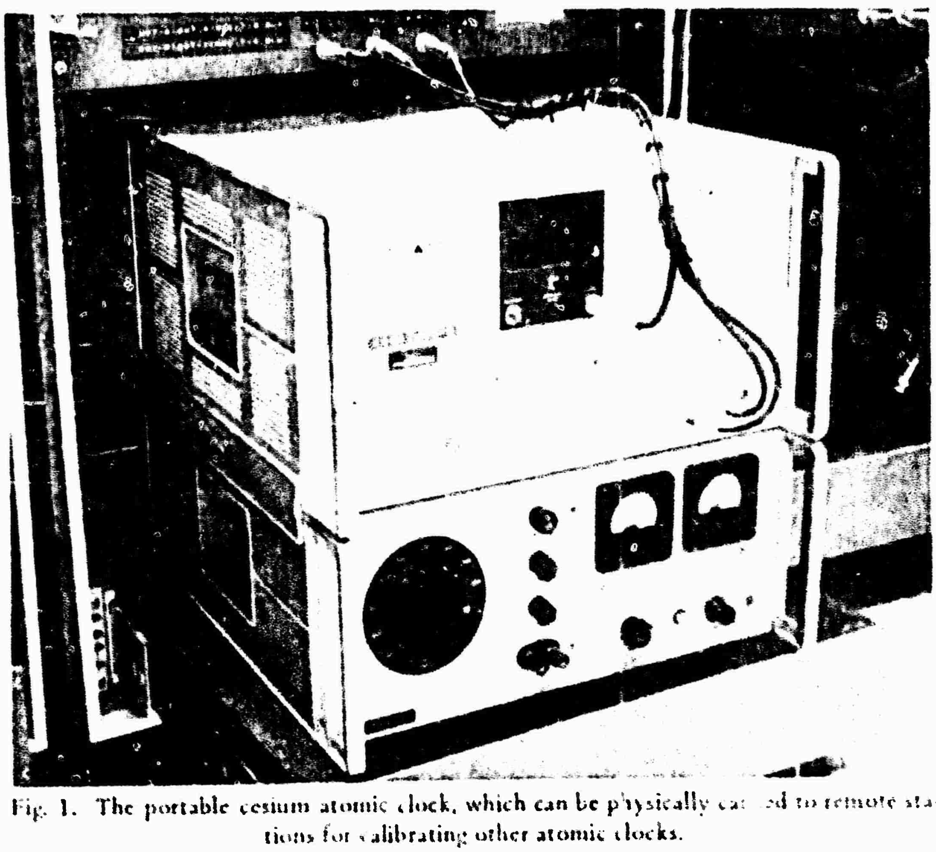


Fig. 1. The portable cesium atomic clock, which can be physically carried to remote stations for calibrating other atomic clocks.

bration of extended systems. There are sc many error sources that one has to have some way of calibrating systems overall. This was one of the clocks used in 1972 when Hefele and Keating made their really pioneering flying clock trip around the world and established the reality of general relativity effects on physical clocks. By flying eastward, then flying westward, there has been a predicted and an observed consensus of four clocks, an observed time difference which is asymmetric. That gives a good picture of the capability of flying clocks about eight years ago. I think today the experiment could be repeated with about the same effort and would probably yield better results by a factor of five. Why? Because atomic clocks become better and eventually they can always, in principle, be made better. In the reduction of the observations of distant signals with propagation delays, on the other hand, we face some very inflexible limitations.

Clock Performance

Now let us look at the performance of some clocks. Figure 2 shows the typical performance of a portable clock. The horizontal axis is days, and the vertical axis is nanoseconds. You can see here the typical random walk of a clock. At one point the clock was absent for a short local portable clock trip, and one can see that there is a systematic offse when the clock came back due to the shock of temperature change and the magnetic environment. There has been a small systematic effect, but overall that is a good example of what today's clocks can do. It is also a good ample of one of our present limitations, namely, the short-term instability, the up/down phase vari-

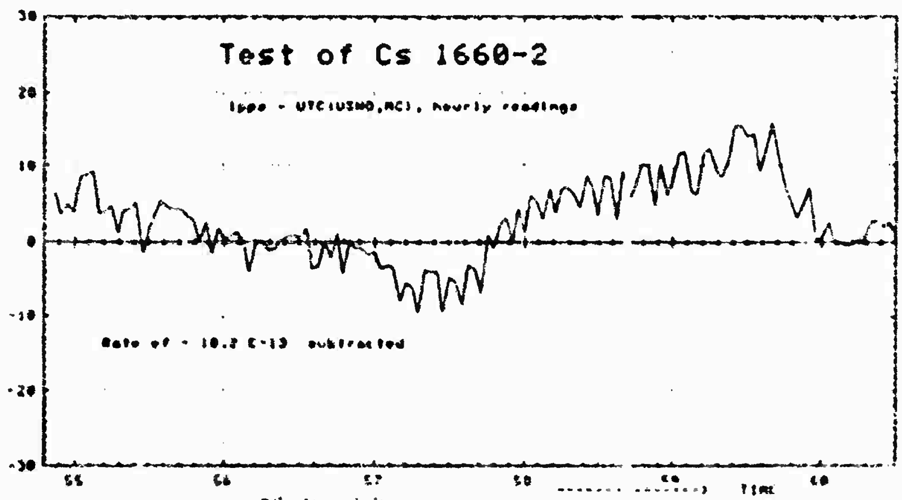


Fig. 2. Typical performance of a portable cesium atomic clock, showing short term instabilities over five days.

ations. Unfortunately, I have to admit they are in our reference. Our references are excellent over five-day and ten-day intervals, weeks and months. The computed time scale has an uncertainty of about one nanosecond from day-to-day, but we can reach that level only after the fact. That level of accuracy is not available in real time, and for that reason we are embarked over the next couple of years on a major effort to update the Master Clock by adding oscillators of very high short-term stability. By having better flywheels, so to speak, our existing long-term stability can be brought to bear immediately in real time on our measurements.

Figure 3 depicts another test of a clock. You see that it is plagued by the same short-term noise, where short-term noise means changes from hour to hour, or from two to three hours. Overall you can see the characteristic performance of these clocks approaches what one calls a random walk. In fact, a random walk in phase or time error is the best one can expect of a clock. If all of our clocks would only produce a random walk in time or phase, we would have time capabilities 10-50 times better than we really have. What we lack is complete control over the environment and over some aging and spontaneous changes in these clocks. The more harmful systematic (correlated) rate variations are caused by these factors.

This brings me for a moment to a discussion of the statistics of these fluctuations in rate versus time. If one has a purely random behavior in the basic frequency determining element, then you expect a variation that is one over the square root of tau (where tau is the measurement interval), i.e., a

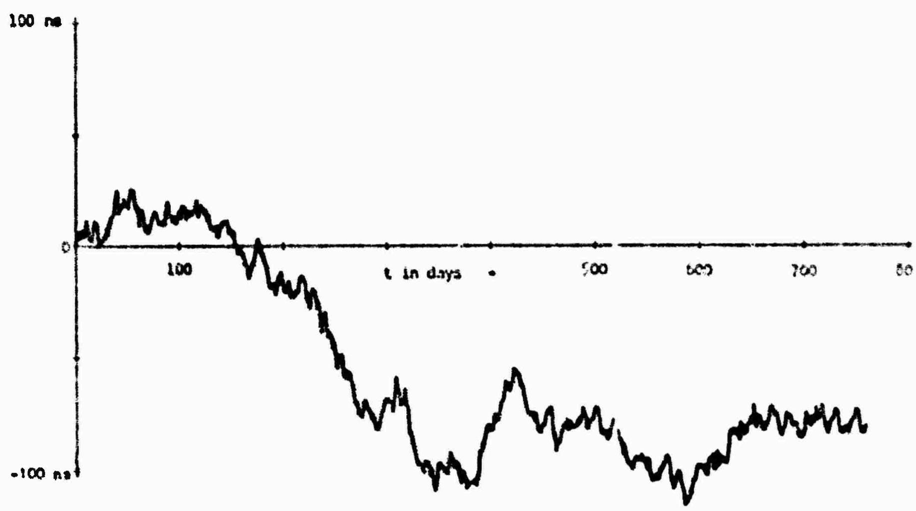


Fig. 3. Performance of a cesium atomic clock, showing the "random walk" effect over 800 days.

double log plot with a slope of minus one half. That would be the best one can expect. Unfortunately, one cannot extrapolate, and eventually there is a random walk not in phase but in frequency over some long time interval. One can see in Figure 4 what we gain by adding very high performance short-term clocks to our system. Instead of having to rely on measurements over increasingly longer time intervals as you go down below the 10^{-13} to 10^{-14} range, and which we could at the present time obtain with a single cesium only after ten days, by the addition of hydrogen masers such precision is available immediately after only 100 seconds.

Figure 5 depicts a typical very long time performance of our clocks; the clock there has been in operation for eight years in its location. That is another example which shows the importance of long-term systematic variations. A better understanding and a better control of these long-term systematic variations will produce a greater performance improvement than is possible with ever more sophisticated methods of computation.

That leads me to mention a substantial effort with respect to our Master Clock room, where we have two time reference systems. The question arises:

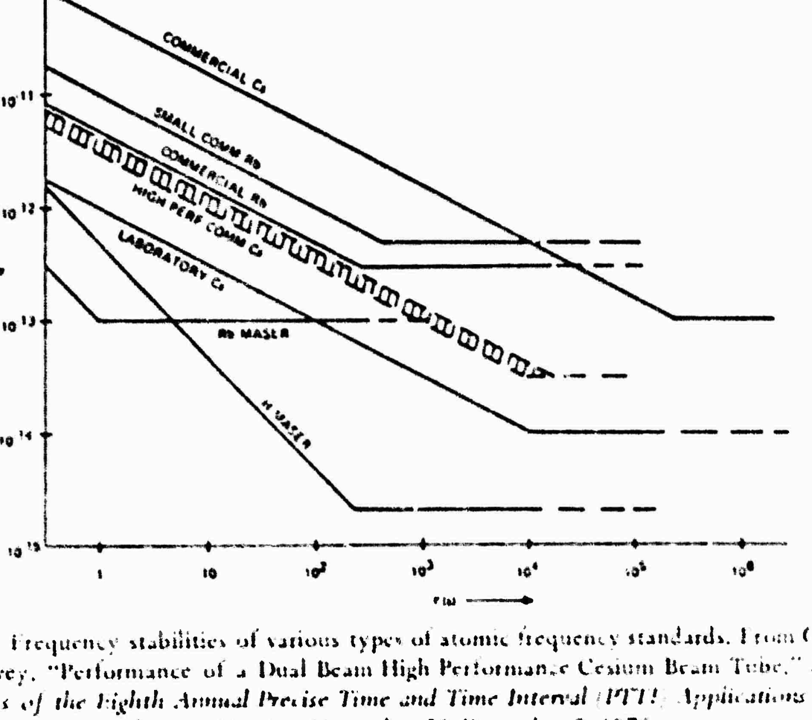


Fig. 4. Frequency stabilities of various types of atomic frequency standards. From Gary A. Seavey, "Performance of a Dual Beam High Performance Cesium Beam Tube," *Proceedings of the Eighth Annual Precise Time and Time Interval (PTTI) Applications and Planning Meeting*, November 30–December 2, 1976.

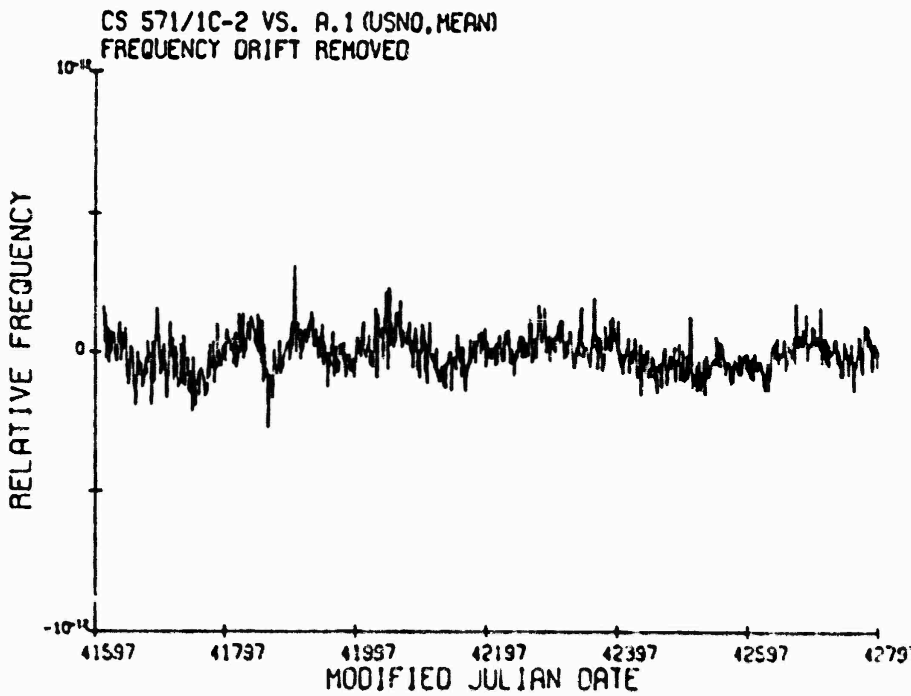


Fig. 5. Performance of a cesium atomic clock over 1200 days.

"How do you control such a reference clock standard?" The answer is that it is done by computer. You depend over a day on a prediction of its performance, and you are again involved in statistical estimation theory.

In Figure 6 we see our main clock room with the monitor part that is to the left. You see our instrumentation just in support to VLF (very low frequency) and OMEGA time of arrival measurements. I expect that we will have considerable changes here. We will rely more on remote measurement of remote transmitters than on local Washington operations. There is no question that we would have long discontinued the monitoring of low frequency signals had it not been for their ever increasing application in communication and navigation. The remote monitoring is leading us to more emphasis on the critical processing of data, which is currently under the supervision of Laura Charron. Essentially, one monitors by collecting teletype messages or, nowadays, by direct remote monitoring of the distant clock through a data link. We must also issue messages to these stations, and disseminate any corrections in real time to the prospective user who needs that information. All of this supports my initial claim that data communication and vastly increasing data processing in real time will be our main problem in the immediate future.

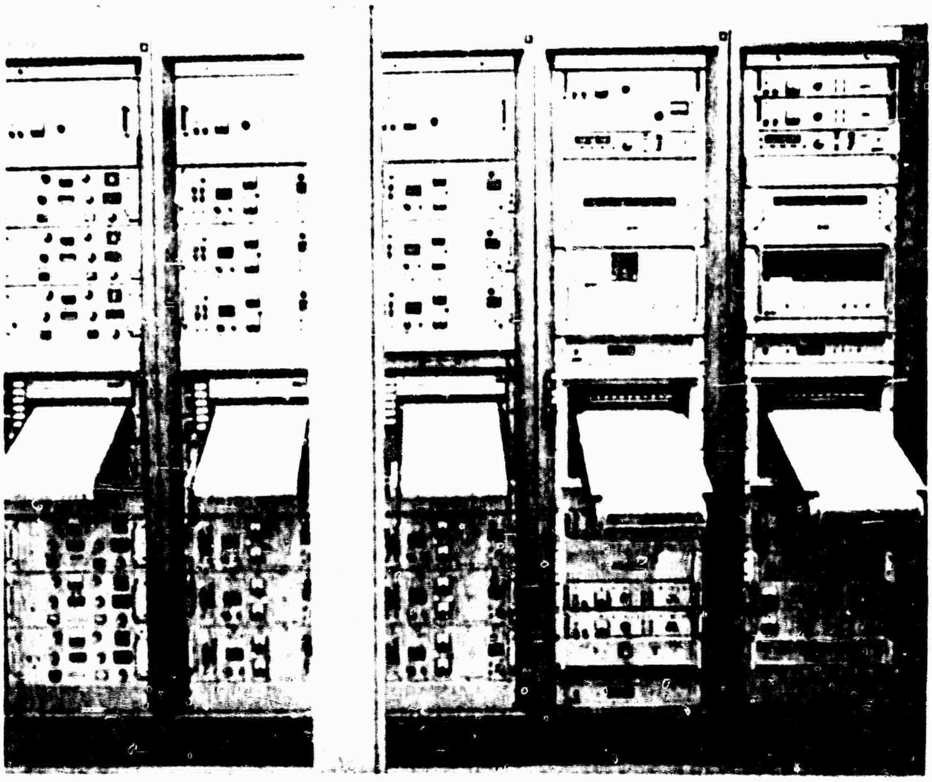


Fig. 6. Main clock room of the Time Service Division, showing atomic clocks (right) and monitors (left) in support of VLF and OMEGA time-of-arrival measurements.

Let me speak briefly about a new navigation system, the Global Positioning System (GPS), which will consist of 18 satellites, each having an atomic clock aboard. In February 1980 we observed the pass of one of the satellites, and while we observed it the satellite was uploaded with data and received a new navigation message, a new orbital prediction, a new ephemeris and a new clock formula. Figure 7 shows this upload in the form of a shaped curve that represents the ten nanosecond change due to changed parameters. We have recently found the clock formula to be somewhat of a problem. Figure 8 is an example of our GPS monitor report, which is available daily; i.e., you call in on the telephone and it is one of about 200 monitor files that you can access. That particular file is filled every morning with the satellite passes of the past day. Figure 8 shows passes of satellites 8, 6, 4, 9 and 5 which came relatively quickly in succession. They had been uploaded just one hour and forty eight minutes ago, and the times that we compute for the differences between the Master Clock and system time are shown. The different satellites give values that are within about 25 nanoseconds of each other;

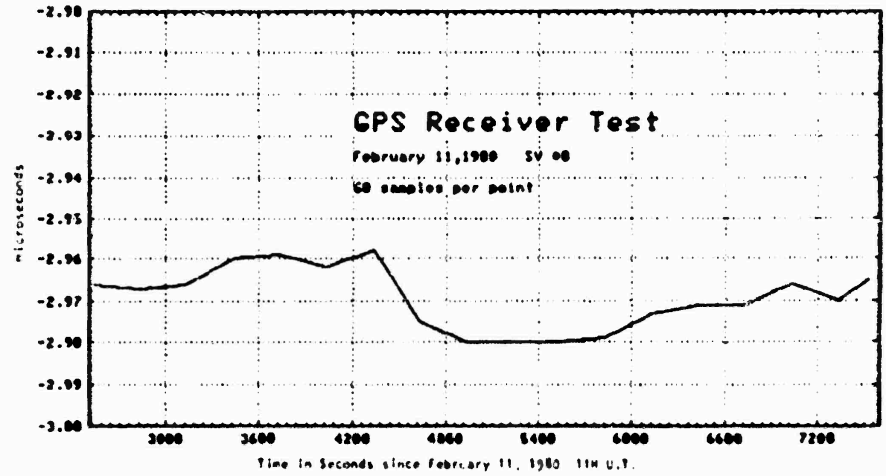


Fig. 7. Global Positioning System satellite receiver test, showing the 0.01 microsecond effect of changed parameters due to a new navigation message sent to the satellite.

that gives you an example of the capability of the system. The timing available after such an upload is, with averaging, within aboat 15 nanoseconds.

Figure 9 shows the behavior of a rubidium standard, in which we see the typical long-term drift characteristic of a rubidium clock. Superimposed are also the up/down phase changes due to erroneous or imperfect orbital calculations. The time aspects of the GPS is an area where we are intimately involved with questions of how to read the remote clocks, how to account for propagation delays, how to set up the system so that we have a minimum of disturbance when the system switches from one monitoring reference to

DATA COLL START MJD 45223 — WKDAY 6 SA DAY/UT 254 20 2 15
DATA PROCESSED MJD 45225 — (UT)12 56 PM MON , 13 SEPT. 1982

SV#	BEG.TRK DDDD.ddd	BEGTRK D HHMMSS	TRKTIME SSSS	MC-GPS US	SLOPE PS/S	RMS NS	SAMP N	ELV	AZMT	D AGE DHHMM	MC-SAT NS
8	5223.835	6 200218	220	-36 505	-15	9	39	70	288	00001	-305615
6	5223.839	6 2007[illegible]	216	-3[illegible] 496	-12	14	37	45	324	00006	646974
8	5223.910	6 214954	492	-36 501	-3	14	83	42	37	00157	-306857
8	5223.917	6 215954	552	-36 503	-5	19	93	39	41	0021[illegible]	-306925
6	5223.924	6 221030	516	-36 505	5	12	87	52	218	00226	647405
4	5223.931	6 222030	516	-36 549	30	12	87	52	299	00240	291934
5	5223.941	6 223424	522	-36 470	-4	11	86	34	281	00141	-41350
9	5223.975	6 232330	516	-36 486	3	13	87	41	253	00239	-62750
5	5223.982	6 233330	1074	-36 478	0	12	179	48	315	00249	-41361
5	5224.034	0 004854	552	-36 496	2	14	92	61	16	00412	-41391
5	5224.086	0 020354	552	-36 501	-11	15	92	54	91	00033	-41409
5	5224.132	0 030954	552	-36 510	-8	14	92	31	110	00147	-414[illegible]

Fig. 8. Global Positioning System monitor report from five satellites identified in column one. The column "MC – GPS" gives the difference (in microseconds) between the Master Clock at the Naval Observatory and each listed satellite in the system.

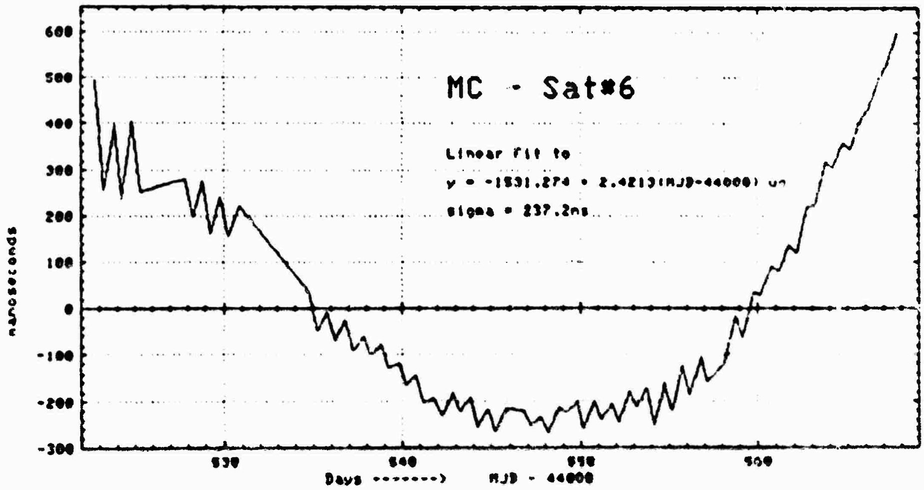


Fig. 9. Difference between the Master Clock of the Naval Observatory and a rubidium clock on GPS satellite 6, showing the long-term drift characteristic of the rubidium clock.

another. We plan to set up monitoring and remote measuring where we can via telephone. We do that now with several experimental sites, one at Patrick Air Force Base, Florida, which we read about five or six times a day and send a message through telephone lines telling them what the clocks are with respect to the Master Clock here. The same method is also used for the communication satellite injection point in Fort Detrick, Maryland.

Determining and Coordinating Time

Let us now turn our attention briefly to the rotation of the Earth. We are concerned here with two entirely different things. First, there is the position in space of the Earth's axis and the polar motion of the axis relative to the Earth. Secondly, there are the variations of the rotation rate of the Earth around its axis. Unfortunately, there are additional local disturbances. One should really compare the Earth with a large plastic surface, a basketball that allows some lateral surface motion over and above the local effects of atmospheric disturbances and gravity variations.

The instrument which has been the backbone of our observations from 1915 to the present is the Photographic Zenith Tube (PZT). It consists basically of an inverted telephoto lens that forms an image, after reflection in a mercury pool, of stars near the zenith. This instrument is in the building shown in Figure 10. It is now succeeded by a large 65-centimeter telescope (f=13m) with major new features. It has a lens cover opening that is very much larger and allows a free flow and equalization of air. The lens is so large (I believe the largest four-element lens in existence) that its thermal

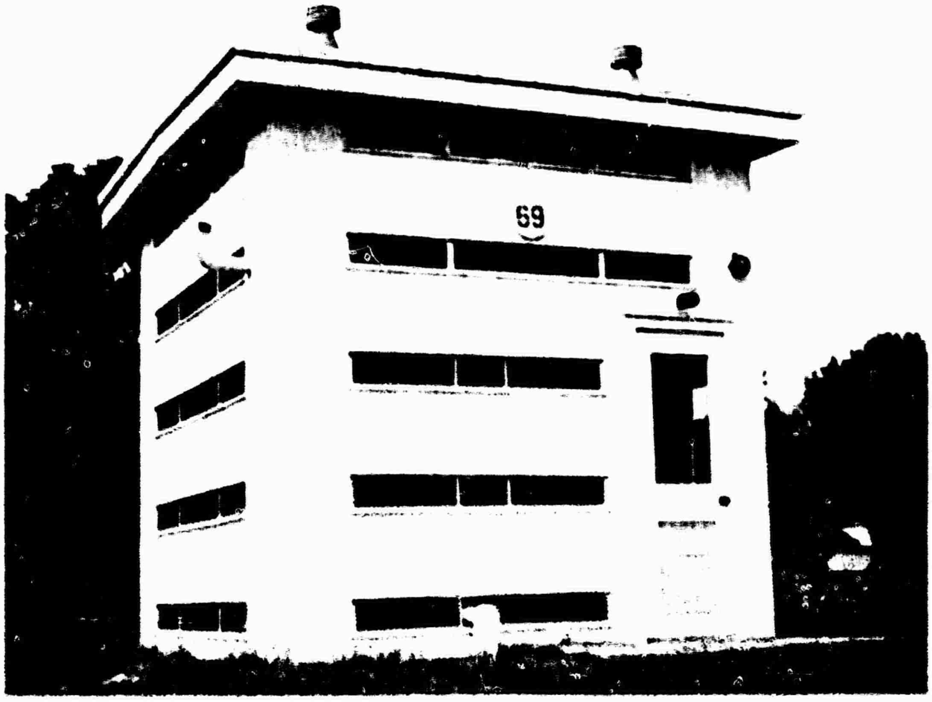


Fig. 10. Building housing the Photographic Zenith Tube (PZT). This telescope is used to determine universal time.

time constant is about 13 to 15 hours, i.e., it has to be pre-cooled for the night. The thermal stabilization of the instrument is therefore a major problem, but I do not think it is an intrinsic difficulty. Dennis McCarthy, who is in charge of the observations, reports that the images are excellent when the instrument is in thermal equilibrium. Figure 11 shows a PZT photographic plate. The bright star is not a PZT star; it is Vega. PZT stars produce typically much smaller images which can be measured with greater precision.

I would like to say a few words about the principal limitations of the PZT and why we have changed and will change still more the relative emphasis between optical astrometry and other methods. The optical method is the result of many years of experience. We observe the same stars every night automatically in four positions, and obtain the time and polar motion routinely. Whereas our observations used to tell us what clock time was every morning, now clock time tells us what rotational position has been taken by the Earth. The limitations are principally instrumental and environmental: the image may not be round; it may be displaced because of the systematic

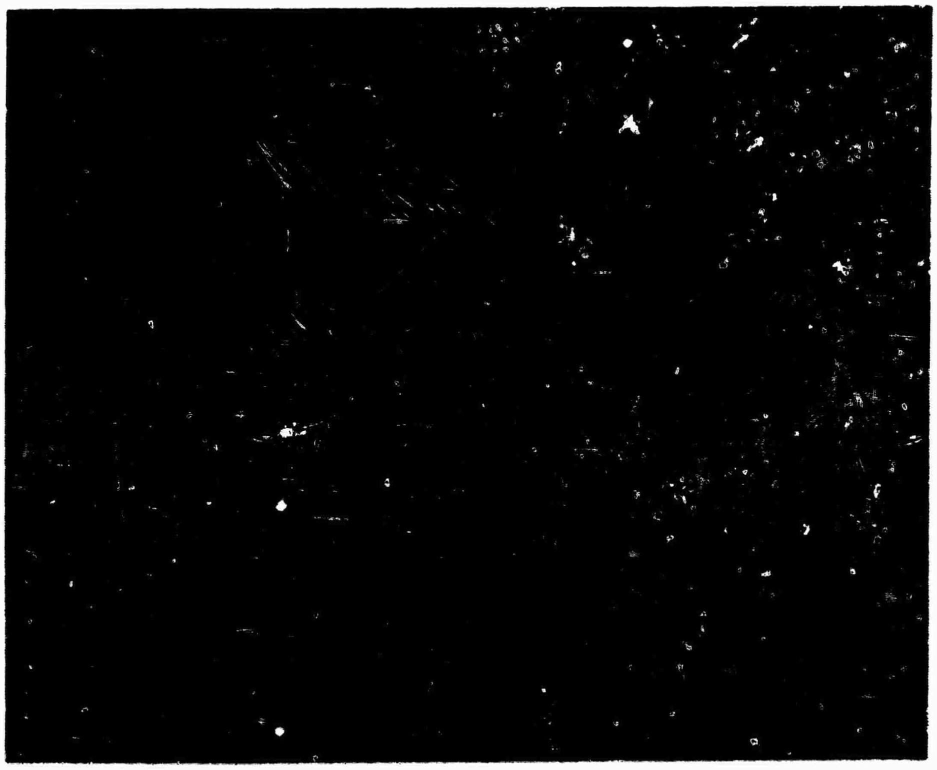


Fig. 11. PZT photographic plate showing images of stars. Four images are obtained during each pass, with the plate rotated 180° between exposures. Images appear in bands due to the different declinations of the stars.

effects in the atmosphere that are characteristic for the near environment of the instrument; and the image may be displaced because of the color effect of different stars. Deflections from the vertical must be taken into account. Vibrations of the large mercury basin have been initial problems, but they have been overcome. With the new instrument we will reach about a one millisecond precision on the nights of observation, based upon a much larger set of stars, about 1200 compared to the 80-100 that we now observe with a smaller instrument. However, I think the main result will be in producing over the years, in a zone 1° wide around declination +39°, very accurate optical positions of stars down to magnitude 11. But there is no doubt that we have reached a limit of the classical (optical) astrometric methods.

For these reasons, four years ago a team headed by Bill Klepczynski, with some very valuable cooperation from other people here at the observatory, embarked on a radio astrometry program in cooperation with the Naval Research Laboratory. This team is using a dedicated instrument in West Virginia, a connected link radio interferometer. We use one common local

oscillator. By means of a microwave link the time of arrival of incoming wave fronts can be directly correlated in near real time. The baseline or the largest distance to one outlying antenna is 35 kilometers. The instrument has great operational advantages, and it has been in continuous operation during the last three years. We expect the utilization of that instrument to continue; in fact, major improvements are being planned for the next couple of years. Our goal is to produce daily reference values for UT and polar motion which can satisfy all critical operational requirements in real time, i.e., we must be independent from other sources.

Now let us talk about international coordination of time for the remaining few minutes. The Bureau International de l'Heure (International Bureau of Time, BIH) is our benchmark. About 50 observatories contribute data to it. Our contribution in clock time is still a major one, like 24 percent (it was in the past as large as 45 percent). That has had the effect that we almost never need to make a coordination adjustment. It is very difficult, risky and inconvenient for thousands of cesium and rubidium clocks to follow deliberate changes in rate that we introduce. Such changes could only be propagated with delays, whether through portable clock visits, monitor results or whatever, and would inevitably generate waves through the system of synchronized clocks. Therefore, we want absolutely to minimize such changes. In fact, the last small adjustment of one part in 10^{13} was introduced four years ago and we have stayed within one microsecond of the BIH ever since. In addition, there are some very small up and down motions, which amount to one half microsecond per year. These are seasonal and originate in the timing links. Some seasonal effects on clocks have also been detected. Overall, we have had much fewer adjustments than some of the other observatories who have had to introduce offset adjustments twice a year.

In Table 1 you will see that we are not doing very well in average weight per clock. In fact, we are trying to achieve a large contribution by brute force, by putting every clock we are testing into the system and automatically sending the data to BIH. This way we accomplish a high contribution, but we could do even better if we could raise our average weight per clock to something like that achieved by the smaller laboratories. They have fewer clocks but can take better care of them. That is one of the areas where we are very much deficient compared, for example to the Physikalische Technische Bundesanstalt (PTB) of Braunschweig, Federal Republic of Germany, which has a consistently higher contribution. This is a consequence of our operational philosophy. Since our staff is too small to afford physicists, we only operate clocks as undisturbed "black boxes". We weed out poor performers but cannot spend the effort to go into the clocks' physics.

Table 1. *Contributions to BIH (effective date, MJD 44479)*

Laboratory	No. of Clocks	Weight per Clock	Total Contribution
U. S. Naval Observatory	27	59 %	23.9%
French group	10	65.8	9.9
Physikalische Technische Bundesanstalt	7	87.9	9.2
National Research Council	6	100	9.0
Royal Greenwich Observatory	7	85 0	8.9
National Bureau of Standards	8	65.6	7.9
Observatorio Marina San Fernando	5	66.4	5.0
All others (16 laboratories)	45		26.2

There is, of course, no benchmark for time available in real time, that is to say, immediately. The benchmark becomes available only two months after the fact when the BIH Bulletin comes out, and we know that two months ago we were 0.3, 0.4 or 0.5 microseconds away. So we have a benchmark that tells us two months or even longer than that, after the fact, where we should have been, and again this means prediction. This prediction takes account of the clock noise and the propagation noise across the Atlantic, which amounts to 0.2 microsecond. We expect major efforts in the next few years from the major laboratories -U.S. Naval Observatory, National Research Council (Canada), National Bureau of Standards, the French group and the PTB- that will probably reduce the noise by a factor of ten. Experiments to do that are already going on. There is a major laser effort underway because laser time transfers are intrinsically accurate. Here we enjoy the cooperation and support of the University of Maryland. Support does not mean that they are paying the bill; unfortunately, we are. But our association with the University team under Professor Alley, with whom we have conducted many experiments in the past, is going to continue. The LASSO experiment, making use of laser satellite retro-reflectors, is expected to begin around September 1982, and will provide, at least for a period of a year or so, a synchronization capability with a one nanosecond precision. Other experiments involve Very Long Baseline Interferometry (VLBI) time transfer and utilization of communication satellite channels, which we are very actively looking at.

Let me say a word about our relationship with the National Bureau of Standards. NBS provides the national standard for frequency. The unit of time, the second, has been defined since 1967 by the International Committee for Weights and Measures as a transition frequency of cesium. The day is therefore only approximately equal to 86,400 seconds, since the small difference due to the variable speed of rotation of the Earth eventually accumu

lates to one second, the leap second. From this follows the need to monitor the rotational position of the Earth (i.e., the difference UT1-UTC), including the motion of the pole, which, among other applications, must also be known for the reduction of the observed UT0 to UT1.

The Observatory's responsibility is to serve as official time reference for the timed systems in the United States. Examples are the navigation systems LORAN, Omega, Transit, GPS, and communications systems such as VLF and the Defense Satellite Communication System (DSCS).

It is clear that the measurement of time and frequency is very closely connected. Yet we find with very minor exceptions that we at the Observatory have an entirely different thrust, because at NBS the emphasis is on the development of new physics for clocks. Our major problem is that there is no long-term guarantee for the atomic time scale reference. It is important for astronomers, more than anyone else, to stay constant in clock rate within one part in 10^{13}. After a couple of years this does make for differences of microseconds, of importance for pulsar research and so on. Absolute measurement accuracy must be improved, and that is where the major thrust goes at NBS. In the case of the Observatory, our concern is with epoch, i.e., time of day. In addition, in our work we have so many stations and so many contacts every day that the atmosphere in Time Service is very hectic. The management of precise time and time interval activities is a vast and fully consuming effort. Nevertheless, pertinent research and method improvements are indispensible and must be carried out concurrently with daily operations. Thank you.

Q: Do you anticipate any radical changes in clock technology?

Winkler: I would say not if you emphasize the word "radical". The problem of atomic timekeeping is how to keep an atom in its metastable state, which is necessary to ensure a very narrow line width. How do you keep an atom in such a state for seconds or longer without it being disturbed? You cannot fly it through a beam for many seconds, unless it is extremely slow. That has been tried and, of course, they fall to the ground and never meet the beam optics, unless you encapsulate them into a buffer gas or in a cell, but then they hit other atoms and get disturbed. So the problem is how to keep these metastable transitions alive so that you can integrate your measurement over a long period of time.

A new scheme is to not use atoms at all but ions, and to pack these ions in an appropriate field. This is entirely possible. A single ion would have the same or very similar properties as the alkali metals that are used now (cesi-

um, rubidium, hydrogen and so on. These are the presently used clock atoms. Many people, including myself, feel that the ion storage clock is a device that in 10-15 years may be even a candidate for a new definition of the second. But in the meantime we feel that the cesium beam is not yet fully exploited. The fact that even commercial standards have been shown to produce for many years the same high quality performance as a long beam machine, shows that the major problems are now of production and quality control, not of design. Our main problems in timekeeping today and in the application of time are mundane things like batteries and power failures and stupidity.

Q: Nearly twenty years ago Markowitz and several others published a paper on the measurement of the cesium resonance frequency in terms of ephemeris time. Is that value superseded?

Winkler: No, that has been adopted as the international standard of time.

Q: It is no longer considered provisional?

Winkler: No, the unit of time is the duration of 9,192,631,770 cycles of the particular resonance frequncy of the cesium atom. There have been several studies after that, mainly by the Royal Greenwich Observatory group, but they have all been well within the errors of measurements. That is something which anybody who has ever seen that old instrument will never understand —how it was possible for Markowitz to come up with that incredibly accurate result. It was a great feat, of lasting importance, there is no question.

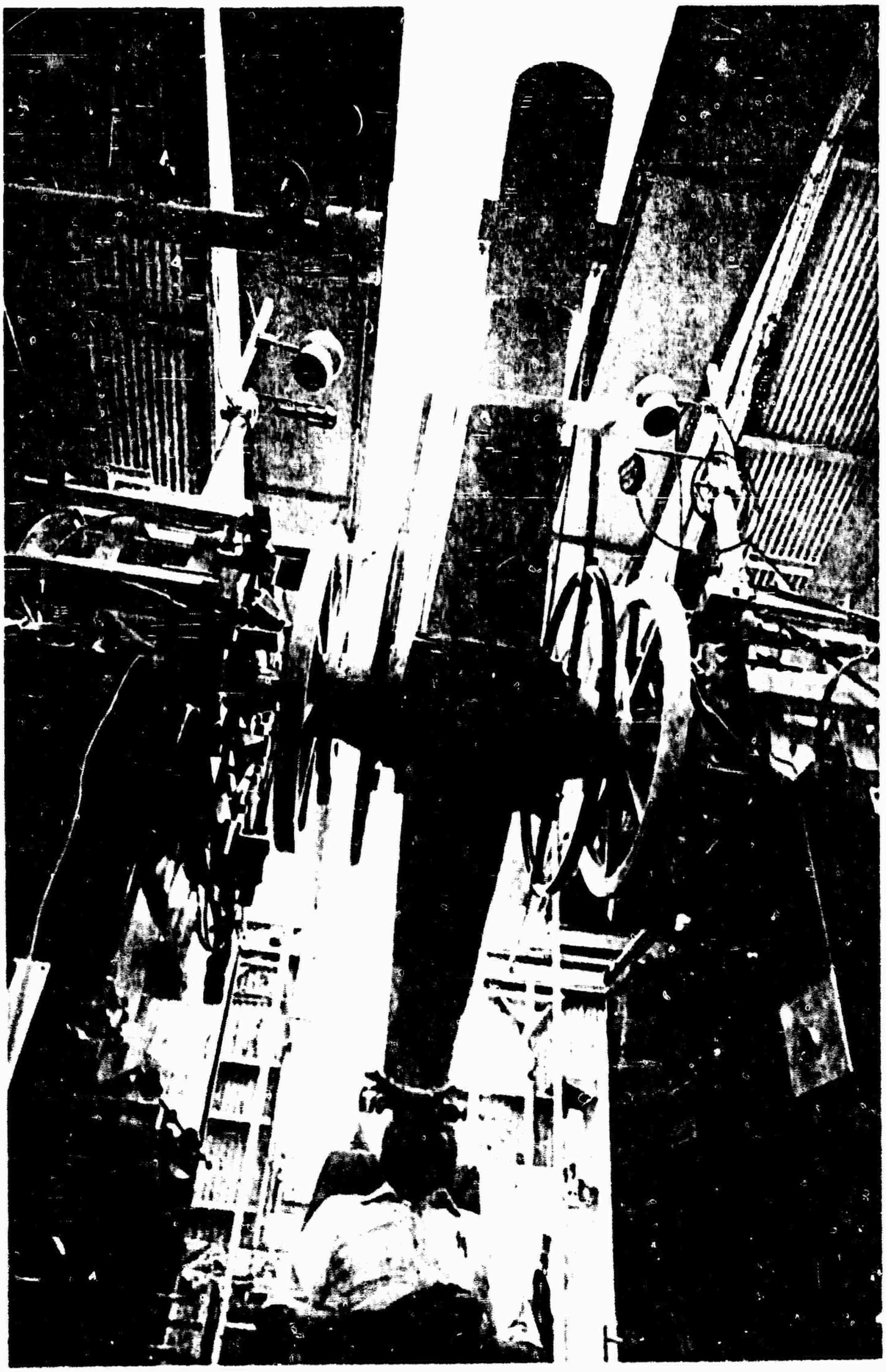


Fig. 1. The six inch transit circle, with R. W. Rhynsburger at the eyepiece. Since 189[illegible] more than a half million observations have been made with this instrument.

ABSOLUTE ASTROMETRY, NOW AND IN THE FUTURE

James A. Hughes
Director, Transit Circle Division*

I am here to talk about the future of the transit circle. I'm a little worried about that, because we are the only division that already has an historian on our staff. However, we've heard today about the mission of the Naval Observatory. The message in the case of the transit circle is much the same as it was at the inception of the Observatory. The whole story can be told in four points: (1) first class navigation of any type requires an inertial system either directly or indirectly; (2) the fundamental stellar reference system is the only inertial system we have; (3) the fundamental stellar system is based exclusively on transit circle telescope observations; and (4) we are the only supplier in the United States. I will let you write the fifth point.

These statements are all quite true. I am not trying to propagandize or in any way say something that isn't absolutely true. Many times we have difficulty getting these points across to budget people and others. Take, for example, a satellite navigation system. When someone pushes a button and an LED display gives the latitude and longitude, and then a transit circle person says, "you need a better standard coordinate system," they say, "we don't need that, we have this black box." Of course they probably don't know that someone had to define a geodetic datum in which the coordinates of the tracking stations are given, and that in turn the geodetic datum depends on some poor geodesist out there on a cold night on a Laplace station determining fundamental azimuths and deflection of the vertical. The immediacy is not quite the same as it was in the good old days when a fellow was out with a sextant looking directly at the Sun. In a sense we have come a long way, and in another way we really haven't made things so much different after all.

I would like to discuss the transit circle in terms of current operations very quickly, and then in terms of research, enlarging on some of the themes broached earlier by Dr. Westerhout.

*In 1982 the Transit Circle Division was combined with the Exploratory Development Staff to form the Astrometry Division, of which Dr. Hughes was named Director.

In the first place the six-inch transit circle (Figure 1), which is undoubtedly the premier instrument of the fundamental system, is currently engaged in observing zodiacal and FK4 stars. The resulting absolute catalog, the $W6_{50}$, will be, as the name implies, the sixth catalog in the series referred to the standard epoch 1950.0.

You have all heard, of course, about our hoped-for mission to the Southern Hemisphere, in particular to New Zealand. I would like to report on the status of that at the present time. It involves military construction at a site on the north end of the South Island of New Zealand near a town called Blenheim. At the moment we are in what is called the "minimum military construction bill" for fiscal 1982. Now "minimum" might sound bad, but actually that's good. The way military construction works there are so-called bands. We are in the minimum band, which means if DoD doesn't get this as a minimum, we're finished as a country. The next band is basic, then enhanced. We are doing rather well on our construction for fiscal 1982. I must candidly say that there are some difficulties with personnel, but we will cross that bridge when we come to it.

But why are we doing this project? As many of you may know, the last fundamental, and I repeat the word fundamental, astrometric observations in the Southern Hemisphere were obtained around 1948 by the Royal Greenwich Observatory at the Cape of Good Hope, which has of course since closed. Now it is true that we ourselves had an expedition in Argentina at Leoncito in the late 1960s and early 1970s, where we did manage to sneak in a quasi fundamental program which gave us some information. But we really showed that the southern sky is indeed in very bad shape from the point of view of a fundamental coordinate sytem. We hope to send the seven-inch transit circle and the twin eight-inch astrograph which is presently engaged here in the zodiacal program, which I am sure Dr. Routly will talk about. These are to be deployed in New Zealand, at a latitude of about 41°.5, this being favorable for above- and below-pole separation of unknowns. We envision having about ten years of observing time. When Table 1 was made the program was to start in fiscal 1981, but you see it has now slipped to fiscal 1982. But it does look rather good.

Although we talk in terms of a Southern Hemisphere program, we ought to bear in mind that the Southern Hemisphere program will benefit the Northern Hemisphere just as well. Because after all, what does a fundamental transit circle program do? It determines essentially, the celestial pole, equator and equinox. Forgetting about the equinox for the moment, the former two are, we hope, 90° apart. But then to the south of the equator a fundamental catalogue observed in the north just trails off. Like the proverbial

Table 1. *Southern Hemisphere Astrometric Program*

A. Why?
   1. Last fundamental astrometric observations in the Southern Hemisphere were obtained in 1948 (RGO-Capetown, since closed).
   2. Southern errors are much greater than northern errors.

B. What?
   1. A transit circle and an astrographic camera deployed in New Zealand (favorable latitude) for ten years.

C. When?
   1. Observing program starts in FY-81.
   2. Final results about FY-91.

hind leg of a dog, it's an extrapolation that just hangs there with nothing to pin it to. By going now to the Southern Hemisphere, we can observationally determine the south pole and an independent equator, and apply the additional constraint that pole-to-pole must be 180°. It sounds like a triviality, but we have suppressed one whole degree of freedom; that is extremely important, as I'm sure many of you realize. So although we characterize it as a Southern Hemisphere program, we ought to bear in mind that from the astrometric or astronomical point of view we are really talking about something which, as an astrometrist I'm ashamed to say, has never been done before: a simultaneous pole-to-pole fundamental program. We'll chase minor planets up and down the sky, passing them from the six-inch to the seven-inch transit circle and so on.

Another current project we have in conjunction with the New Zealand project support is an attempt to modernize and to some extent automate the seven-inch transit circle (Figure 2). I can only report to you at the moment that things are going reasonably well with this project. We looked at Polaris last week with the image dissector and got some incredibly bad seeing images, which Dick Branham is now fighting with, trying to find if they indeed have a center. We hope they do, and some of the other accoutrements are coming along pretty well. We haven't run into any brick walls; I suppose that is the important thing. Our new circle scanning system is looking good. We had our first test runs on the prototype last week, and we are getting repeatabilities in some cases around the 5-7 micro-degree level. These are pretty good results.

Another current research project is a joint project involving Princeton University, Draper Laboratory and NASA to use the Orbiting Astronomical Observatory (OAO) in its final days of life to make the first large-angle astrometric measurements from space. In fact those are starting even as we talk now. The gyros on the OAO were not meant for this kind of thing, but

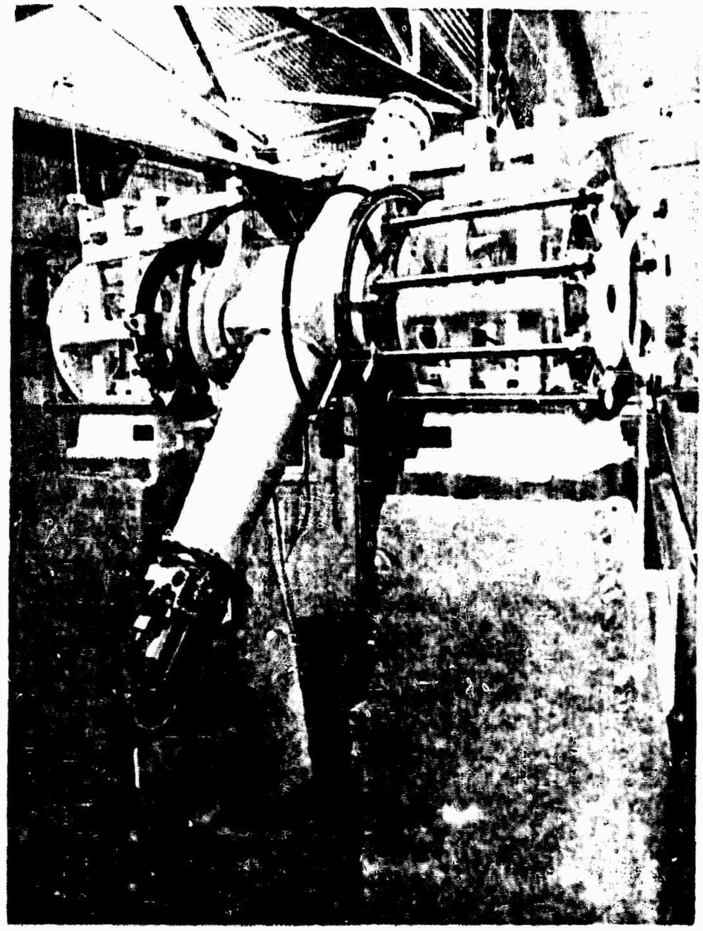


Fig. 2. The seven-inch transit circle. This instrument will be deployed in New Zealand during the 1980s for fundamental astrometric observations of southern stars.

it turns out that they have been performing two to three times above specifications for the eight years that OAO has been in orbit. So we thought this is a good opportunity to see what we can do to demonstrate the concept of measuring large angles inertially in space. We are using a spectroscope slit, and we will only move the instrument in yaw. We have used some of our Southern and Northern Hemisphere data and picked out the really bad actors with regard to position in the FK4. We have set up various triangles, and we will now measure those with the OAO and see if we can at least confirm the ground-based corrections which are indicated by both the Washington/Leoncito results — that's the seven-inch in the Southern Hemisphere that I mentioned earlier — and the latest catalogue from the six-inch, the Washington $W5_{50}$. So this is a contemporary effort.

If we want to think about research in general and for the future, it is our belief that fundamental astrometry can be improved through research in the

areas indicated in Table 2. The first is the study of the atmosphere. It seems in so many cases that this has become the decisive and limiting factor for our ultimate accuracy. We propose in our case to embark upon an investigation of the local atmosphere using LIDAR techniques. For those of you not famililiar with LIDAR, it is an acronym similar to RADAR; whereas RADAR is radio detection and ranging, LIDAR is light detection and ranging. It is an accurate method that uses the backscatter. We are looking at two kinds of LIDAR. One involves Raman backscatter, which is molecularly specific, and in the other kind one looks at the on-frequency backscatter. Here you need a tunable laser which is tuned either right on or off an absorption line of the species under study. Both of these methods have their pluses and minuses. Measurement of water vapor in the atmosphere is done almost routinely using both of these methods, so I really don't consider water vapor quite the problem it has been in the past, that is, out to ranges of possibly four or five kilometers. Higher than that the water vapor drops off pretty strongly anyway, but of course if you are measuring a slant range, you have a slightly different problem. This is a beginning R & D project. It is now in the stage where we are looking for proposals to specify a system, and we hope next fiscal year to actually build it and have it in use right here in Washington.

Coming to point B of Table 2 we have optical and infrared interferometry. The optical is of course the most difficult of all the interferometric methods, since as the wavelength gets smaller the mechanical problems get

Table 2. *Research Areas in Fundamental Astrometry*

A. Atmospheric Studies
   Real time measurement of atmospheric refraction via:
   1. LIDAR (active probing)
   2. Dispersion measures
B. Optical and Infrared Interferometry
   Optical fringe tracking
   Infrared (11 microns) using laser as local oscillator
C. Large Angle Measuring Devices
   Divided circle is presently the most accurate device (about 0."05)
   Inertial methods show great promise
   Possibly a hybrid system involving mechanical and ring laser gyros
D. Dedicated Astrometric Satellite
   Follows logically from "C" above
   No atmosphere
   No gravity
   Possible extra galactic inertial reference frame (quasars)
   Direct measurements made of "optical counterparts" of radio sources

bigger. There is an inverse relationship. However, what is very exciting is the infrared region, as exemplified by Professor Townes and his group working around 10-11 microns. The advantage with these wavelengths is that one can use a laser as a local oscillator and then have a heterodyne system just as one does in radio, and from there on the analysis of the data is precisely the same—the correspondence is practically one-for-one. There are two nice features about infrared. One is that several hundred FK4 stars, according to statistics done by Tom Corbin, are directly observable by this technique, using mirrors of reasonable size. The other thing is that if one looks at the dispersion curves of the atmosphere, one finds that around 10.5 microns they cross (as far as water vapor in the air is concerned). This means that for an infrared interferometric study, you really don't care what the constituents are in the atmosphere, but only what the total density or total pressure is. It simplifies a lot of problems when you do not have to worry about the constituents you are looking through, particularly for greater zenith distances.

Up until this very day, the most precise and accurate observational method of measuring large angles is the classical divided circle. Granted, we use it in conjunction with some very modern scanning techniques, but we are still using a piece of metal, or in some cases glass, with lines scratched on it. The accuracy that we get from this is on the order of 0."05, although our new scanners are even better than that. But we are practically coming to the point where we cannot push that technology much further. There seems to be a brick wall around 0."02 for this kind of technology. But after all, here you have something that is outside in the observing room. It is not in a clean room, not in a controlled environment; it is expanding and contracting with temperature, and so on. It's amazing to me that we do so well, observing as we do in the real world. One of the ways to attack this fundamental problem is to ask the question of how one measures large angles. Inertial methods show great promise. What we are talking about here are the third generation gyros and even beyond, as produced by Draper Labs, which has built the guidance system for practically everything that I've heard of that went into space—that's an overstatement I'm sure. But some of their in-house developments are very encouraging, and we do believe it should be possible to come up with inertial methods that will have a double thrust. In the first place, we can apply them on the ground to our present instruments. Our present instruments will serve as a benchmark, if you will. We know how they work; we know what we can expect from them. If we then apply these inertial methods to present instrumentation, we can test these inertial methods vis-à-vis the old established and reliable methods. If these tests then prove

that we really have something that will work, the next step would be to try to make measurements from space. This refers to what was mentioned earlier about making astrometric measurements from the very place at which they are likely to be used.

Hence point C leads to point D in Table 2. The advantages of doing astrometry from space are rather obvious: no atmosphere, no gravity, and the availability of an extragalactic inertial reference exemplified by the "optical counterparts" of radio sources. I put "optical counterparts" in quotes because we do not know precisely if the optical and radio emissions do indeed coincide. And this is a very important consideration in fundamental astrometry.

Figure 3 shows an artist's conception of an astrometric satellite. Without making too much of this, let me say that what we are trying to depict here is something that is small, simple and dedicated to the task of fundamental astrometry; i.e., large-angle measurements in two dimensions, as opposed to measuring angular separations alone. We could then do several things. For example, the minor planets can be referred to a selected set of stars on a very regular basis just as we do now, except that we now call the stars clock stars. The significance of clock stars is lost, of course, once you are off the Earth.

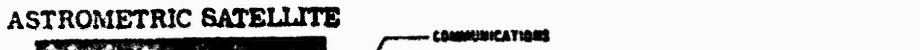


Fig. 3 An artist's conception of an astrometric satellite

But nonetheless we could have that subset of stars to which we could refer minor planets in a very systematic way and do similar reductions, as we do on the ground. By having an integration capability, i.e., a hover and hold capability an instrument such as this, we can extend the dynamic range and look at fainter objects, which is also a very important aspect of the whole problem.

One of the ongoing questions that we have always had around here, at least I have always had, has been just what effect refraction has on an interferometer. I have heard statements like, "it has no effect at all." I say that is absurd. After all, the device measures the dot product of the baseline and object vectors, and if the latter is moved by refraction, the dot product must be affected. I recall myself and other people who shall remain unnamed going around and around in a friendly way on this, but I think Figure 4 indicates what I call my "definitive mistake." The point is, I think it is important that people know that refraction does affect interferometers at any wavelength. However, in the case of interferometers, there is a definite advantage: to the extent that the atmosphere is plane-parallel the effect can-

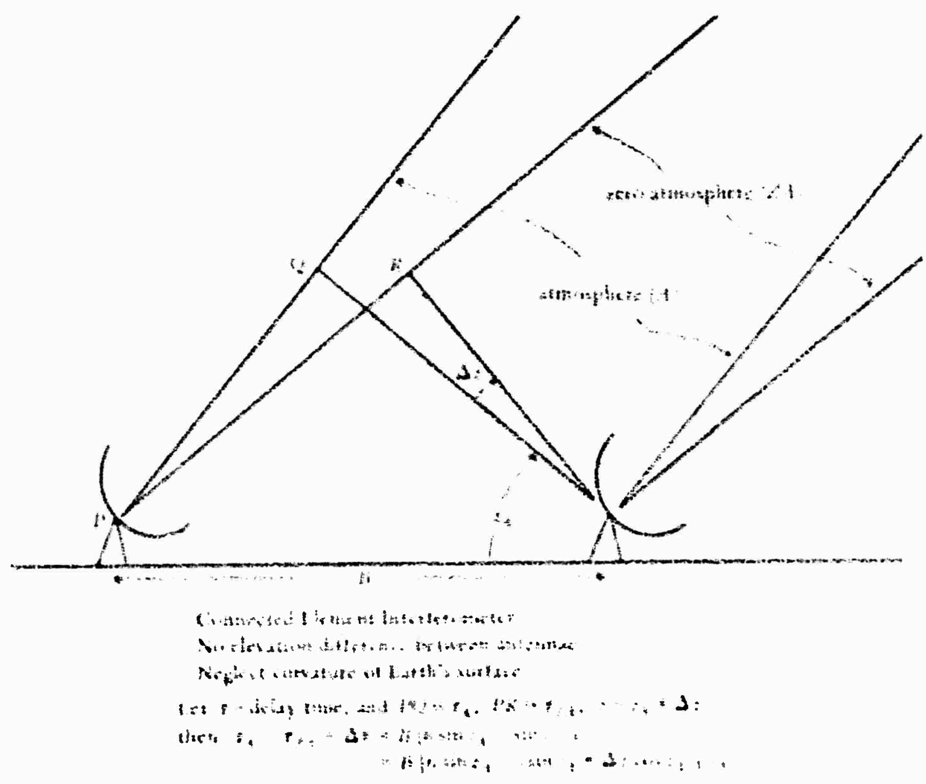


Fig. 4. The effect of refraction on interferometers. (c is the speed of light.)

cels. Unfortunately, the atmosphere is not plane-parallel or dry, and the correction between the plane-parallel and the spherical and structured atmosphere is about a thousand times bigger than the accuracy inherently possible with an interferometer. When the effect is a thousand times bigger than your precision, I don't call it a second-order effect. That is probably the basis of our semantic difficulties.

Since we are the sole purveyors of fundamental astrometry nowadays, not only in the United States, but in the Western World, we find ourselves in a somewhat discomfiting situation as follows. On the one hand — and this has been mentioned by several speakers — we have an operational requirement and the necessity of furnishing data for use now. On the other hand, we must look ahead, and we must not become old fashioned. We must by all means avoid the "not invented here" syndrome and the "we have always done it that way" syndrome. We have to be extremely careful to avoid these. With the restrictions on the number of people we have, we all know the dangers we face here at the Observatory of inbreeding. We have to be careful about that as well.

I suppose this all sounds like a cry for sympathy, but what I am trying to tell you is that the Transit Circle Division is attempting to do two things. On the one hand, we are trying to keep the tested, tried and true methods producing. On the other hand we are trying to look ahead and, to the extent of the resources available to us, trying to see what can be done with some of the new techniques. I am sorry to be forced to say that some claims are being made nowadays that cannot be supported by the facts as far as fundamental astrometry is concerned. This may have been an inevitable consequence for the struggle for limited funds. Please note that I'm not talking here about the Observatory, but about the astrometric community in general. For that reason, we in the transit circle business must be a bit conservative but not too conservative. That's the trick.

N. G. Roman: I have two questions. First, have you looked into the use of superconducting gyros? Do they offer any advantages for spacecraft? Secondly, on the astrometric satellite, are you thinking in terms of a U. S. satellite, a Navy satellite or HIPPARCOS?

Hughes: No, we are speaking in terms of the Navy, because it would have capabilities that HIPPARCOS does not have. As far as the gyros are concerned, what we have really thought about so far is a hybrid system. We have talked about very advanced gas bearing gyros in conjunction with ring laser gyros. We don't like the ring laser gyros alone since you have to dither them.

but a hybrid might be a very good approach. But this is speculative at this point.

R. S. Harrington: (Exploratory Development Staff): I notice you have said nothing about the Automatic Transit Circle in Flagstaff.

Hughes: The name has been changed. It is now the eight-inch transit circle. I finally said, "Look, the optical system as an optical system is very good; but we are never going to stabilize the thing with respect to the pivots, the cube, the cage, the piers and so on." So we took out the entire optical system. I sent to Flagstaff a little four-inch lens that we had pirated from a University of Pennsylvania transit instrument that we had here. We mounted that to the basic tube structure, and went through the same series of stability tests that the instrument had failed with the original optics. It now passed with flying colors. And indeed in some cases it was apparently more stable than either the six-inch or the seven-inch transit circles. Which is to say, mechanically, the cube-tube-cage structure is very good, and all the problems were in the reflecting optical components and in trying to keep them fixed – not with respect to each other, which they did pretty well, but with respect to the barrel. At that point, we had some money left over, which we have used to get an eight-inch lens, a classical doublet. The optical specifications are the same as the present specifications of the six-inch, which performs very well. We hope to have the lens delivered in a few months and then installed. We have done pivot measures and screw errors already. We're going to do circle diameter corrections, and sometime next calendar year that instrument will be ready to go. We already have several programs lined up for strictly differential work. For example, occultation candidates, radio source reference stars, referring the PZT stars to the FK5, and so on.

R. E. Schmidt (Nautical Almanac Office): For your ten-year program in New Zealand, how many people will be down there permanently, and where are they coming from?

Hughes: I would suppose there would be something on the order of a half dozen people required to carry out the transit circle program. We hope to hire some New Zealanders. There are all kinds of mixes of personnel one could envision, depending upon a lot of decisions that have to be made, both within and without the Observatory.

W. E. Howard (National Science Foundation): There is some Australian

astrometry going on. Can you comment on how your program would interact with theirs? Are they not doing it with your sophistication?

Hughes: The difficulty in the Southern Hemisphere is that there is no *fundamental* astrometry going on whatsoever. There is good photographic work and good transit circle work, but there is no comprehensive program, even in the differential case. For example, there is no comprehensive re-observation of the entire Southern Reference Star system (SRS) by transit circles, which certainly is called for. We essentially do not have any idea of the proper motions of the SRS, at least not in any scientifically meaningful way. Pending the arrival of the FK5, I don't know what might happen. But there is no fundamental work whatsoever at this time.

T. E. Corbin (Transit Circle Division): I might point out that although fundamental observations of FK4 stars were last made in 1948, some of the new members of the FK5 haven't appeared in a fundamental catalogue for many decades.

Hughes: Yes, what Tom Corbin has touched on here is something that really concerns us in the fundamental astrometric community. When the FK5 comes out it will have what is called a faint fundamental extension, which is a very welcome thing -- several thousand stars in the seventh to ninth visual magnitude range added to the fundamental sytem. But I can assure you -- and this is what Dr. Corbin was referring to -- that by putting the positions and motions of these stars in a catalogue, they are not going to be of fundamental quality. The FK4 is bimodal hemispherically. The FK5 is likely to be bimodal hemispherically and with respect to magnitude as well.

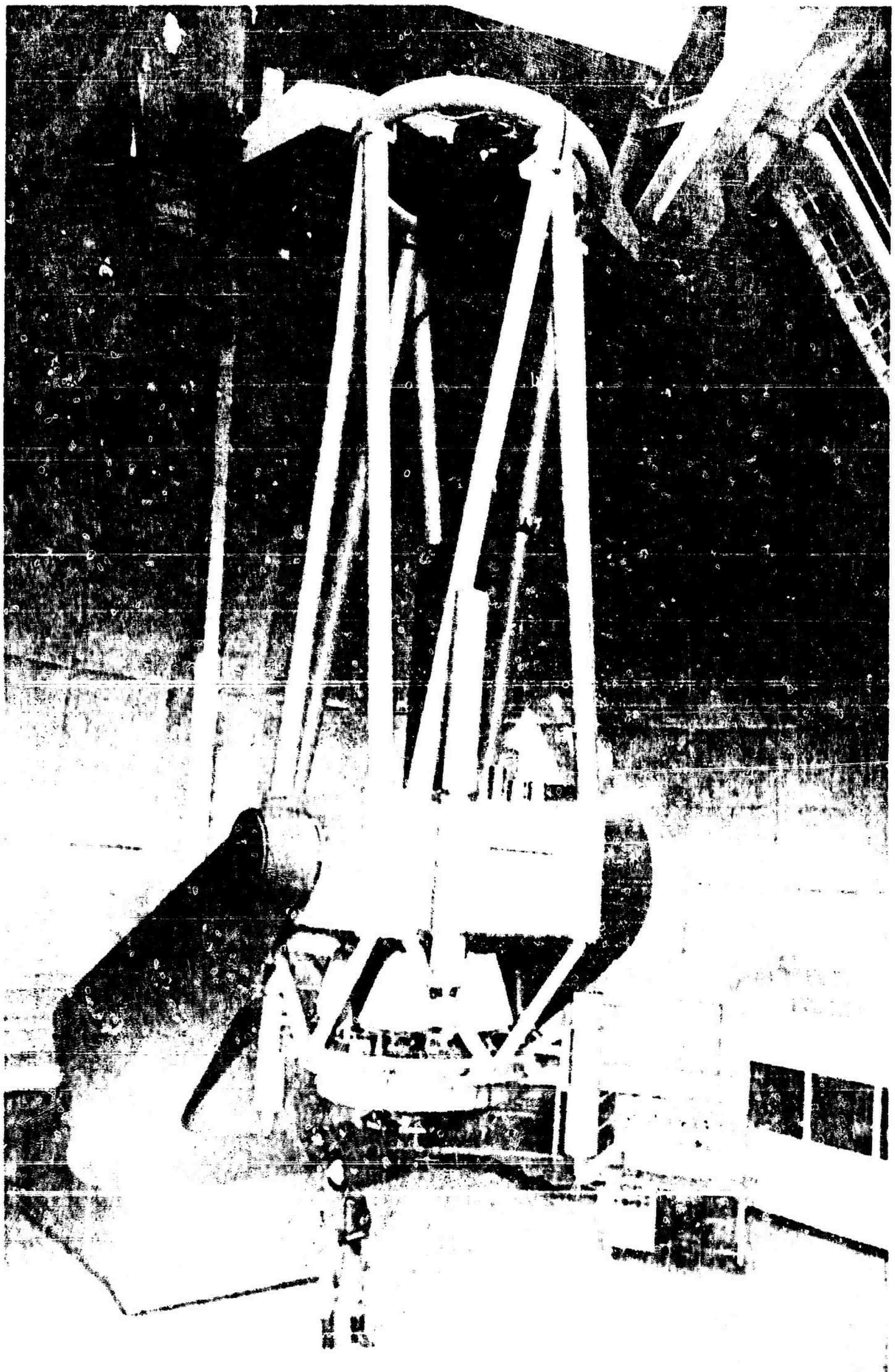


Fig. 1. The 61-inch astrometric reflector at the Naval Observatory's Flagstaff Station. Dr. K. Aa. Strand stands at the base of the telescope.

SCIENTIFIC PROGRAMS OF THE EXPLORATORY DEVELOPMENT STAFF

Paul M. Routly
Head, Exploratory Development Staff*

The programs in the Exploratory Development Staff (EDS) are of a long-range nature, and they feed directly into the other divisions here at the Observatory. They also have a very powerful impact and spinoff in the astrophysical world.

I think the best way in which to describe our work and future plans is to explain the programs themselves in some detail. Let me list the four principal programs in which we are presently engaged:

1. Parallaxes/Proper Motions of Nearby Stars
2. Astrographic Catalog
3. Visual and Photographic Binary Stars
4. Astrometry of Solar System Objects

The parallax/proper motion program, undertaken jointly with the Flagstaff Station, is restricted to faint stars lying in the magnitude range between 12 and 18. This is the largest of its kind in the world by far, and really defines the stellar inch. It is the program that establishes the basic stellar distance scale to which all other stellar and galactic distances are ultimately referred. The astrographic catalogue is a unique effort on our part to come up with a new catalogue of star positions for both the Northern and Southern Hemispheres. It is unique in the sense that the astrographic observations will be carried out in the Southern Hemisphere at the same mean epoch and from the same position as the planned transit circle observations. The third program concerns double stars. We carry out here the largest program in the world on double stars, both visual and photographic. And fourth, we have a program having to do with the astrometry of solar system objects, primarily minor planets and satellites, although comets are also of interest to us.

Let me now very quickly go through each of these programs in turn, describe the origin of the program, what equipment is used, what limitations

*In 1982 the Exploratory Development Staff was combined with the Transit Circle Division to form the Astrometry Division.

apply, what results have been obtained so far, and where we hope to go in the future.

First of all, the basic telescope that is used for the parallax plate-taking is the 61-inch astrometric reflector in Flagstaff. Indeed, I should point out that this program is a joint effort between the EDS group in Washington and the astrometric group at the Flagstaff Station. The manager of the program is Bob Harrington, who is ably assisted here in Washington by Jim Christy and Varkey Kallarakal. The actual observations are made in Flagstaff, and the gentleman out there who selects the parallax stars, runs the program, and carries out all the photometry is Conard Dahn, assisted by most of the staff in Flagstaff.

The parallax program was started in 1964, and in fact the 61-inch astrometric reflector was specifically built to carry out this program. The second instrumental partner in the program is SAMM, the Semi-Automatic Measuring Machine, the first of its kind to be used in astronomy. The basic features of this machine are that it can move around on a photographic plate to positions that are fed to the machine in advance and, most importantly, that it has the capability of centering on stellar images automatically without any personal bias from the operator. Becuase of these automatic features, it is possible to carry out a series of measurements with SAMM lasting some hours without obvious degradation in the quality of the work because of operator fatigue.

Figure 1 shows the 61-inch. I'm sure everyone at the Observatory has seen this photograph many times before. It shows Dr. Strand standing at the bottom of the instrument. He conceived the idea of the reflector and the SAMM measuring engine together as a working system. The 61-inch is a very interesting telescope. It consists of a parabolic primary, but instead of a hyperbolic secondary as you might expect, it has a flat secondary which reflects starlight down the telescope tube through a hole in the primary mirror to a photographic plate behind. The 61-inch is like a conventional Cassegrain, except for the flat secondary; in a sense it can be thought of as a folded Newtonian. The unique design feature of this telescope is that as it assumes various positions in the sky, and as the telescope tube droops from its own weight, the cantilever design of the tube causes the flat secondary to move parallel to itself. The point is that the optical axis of the 61-inch is mechanically stable in the sense that it intersects the photographic plate at the same point all the time, regardless of the telescope position. This is one of the absolutely basic requirements for the instrument to be astrometric.

The magnitude range of the parallax program lies between 12 and 1[illegible] faint stars – because the light grasp of the 61-inch is so great that we cannot

handle bright stars; at least we haven't been able until very recently. This means that the program is really designed to measure the distances to objects like red dwarfs and degenerate stars. In addition to plate-taking, *UBV* photometry is also carried out routinely on all program stars with the 61-inch by Conard Dahn.

The problem of choosing a star with a likelihood of having a non-zero parallax is an important consideration. At first, we utilized the criterion of high proper motion. It was argued that if a star had a high proper motion, then the probability was fairly good that it was nearby and so had a measurable trigonometric parallax. We used for this criterion the two proper motion surveys of Henry Giclas and William Luyten. More recently Conard Dahn has been carrying out preliminary *UBVI* photometry on all candidate stars because he has found the *V*-*I* index correlates strongly with absolute magnitude and gives a better indication of distance than high proper motion.

Once a star has been placed on the parallax program 35-40 plates are taken of it over three or four years. These plates are sent to Washington where they are measured on the SAMM measuring engine.

From this program six individual lists of parallaxes have been published containing a total of 582 parallaxes. The distribution of the parallax stars is pretty uniform, down to about declination −10°, and is good, though not uniform, down to declination −20°. Figure 2 shows the new data plotted on a Hertzsprung-Russell diagram. One can see that an enormous amount of detail has been added to the faint end of the main sequence as well as to the degenerate sequence.

One of the most amazing results that has come out of this work so far and the reason, I think, that the 61-inch is appropriately regarded almost as the standard of the astrometric world these days — concerns errors. After the first lists were published, Bob Harrington had enough data to carry out a statistical analysis of the errors. By using the field stars to determine external error, and by comparing this with the internal error that we were formally calculating all along, we found that these two errors were the same, about 0.″004. The conclusion to be drawn from this is that the 61-inch reflector/SAMM combination is an astrometric device that has no systematic errors at this level. This is a very unusual conclusion, particularly in view of the fact that all parallax determinations prior to the 61-inch (with the exception of a few parallaxes that were attempted on the 60-inch at Mt. Wilson in the early days) were obtained with refractors and show rather bad systematic errors, or certainly systematic errors that are non-zero. So we conclude that for the 61-inch/SAMM combination the internal error is really equal to the true error. This means that within some finite limit, we can decrease the error and

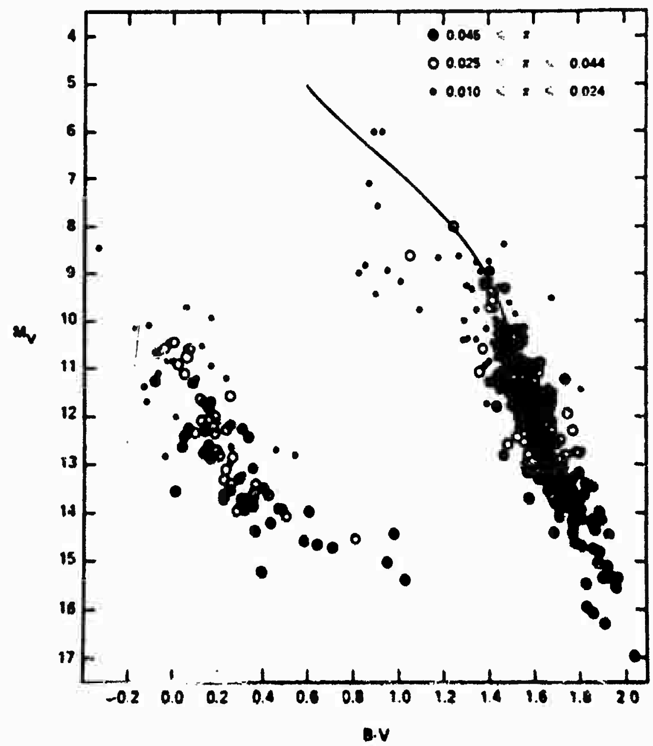


Fig. 2. Hertzsprung-Russell diagram of parallax stars observed with the 61-inch reflector.

improve the overall situation, by simply crunching away and exposing more plates. If, for example, instead of a series of 40 plates, from which we typically get an error of 0.″004, we were to expose 160 plates, which is a heavy burden to carry, though not impossible if the reasons for it were important enough, one could cut this error down to 0.″002. At what level systematic errors will rear their ugly head is not known, but we expect that we will be able to come down to 0.″001.

To summarize, with the present 40 plate series, we have essentially increased the range from 25 parsecs, which is what trigonometric parallaxes were always thought to be limited to in the past, out to 250 parsecs. We are now sampling, or can sample, a much larger volume of space than was possible before. With more lengthy plate series, we should be able to derive astrophysical information about objects such as giants, subgiants, Cepheids and RR Lyrae stars by determining the distances to them directly. There is even some hope that we might be able to determine trigonometrically the distances to the Hyades and Pleiades clusters, which opens up a whole Pandora's box of possibilities regarding the direct calibration of more remote distance scales.

Another very important result concerning bright stars has come out of the error analyses. I mentioned at the beginning that, because of the light grasp of the telescope, we are limited to a magnitude range between 12 and 18. We cannot photograph a bright star with the correct exposure and have at the same time the required number of faint field stars on the plate to be able to measure the position of the bright star accurately enough to produce a parallax. Furthermore, most bright stars are far away and have very small parallaxes, and hence are not ordinarily promising candidates for the trigonometric parallax determination.

We think we are now at a stage where these difficulties regarding bright star parallaxes can be partly overcome. First, we can take many more plates so as to reduce the errors and detect even smaller parallaxes. Second, to solve the magnitude problem, a series of filters is being prepared with vacuum deposited inconel spots which will result in attenuations of one, two, three, four or five magnitudes and so on. The idea is to place the image of the bright star on a spot of appropriate attenuation so that the bright star photographs as though it were similar in brightness to the field stars around it.

Another thing we are going to do is to go to fine grain plates in an attempt to cut down on the plate error. We think we might be able to cut the plate error, i.e., the signal-to-noise problem, by half if we go to finer plates. These plates will have to be sensitized (baked) in forming gas in order to get them back up to the speed of the plates we are now using.

We are also beginning to worry about the connection to absolute parallaxes. So far we have been talking about relative parallaxes only. With the accuracy capability of the sort being described here, however, one can actually try to measure the parallaxes of quasars or galaxies. These, of course, should be zero. If a small non-zero value is obtained, this could be regarded as a correction to absolute for that particular parallax field. If enough such quasar parallaxes could be obtained, they would help to calibrate the statistical corrections now applied over the entire sky.

Finally, the question of accuracy has now reached the point where it will be possible — and there is an IAU committee working on this problem right now — to choose a set of stars whose parallaxes are the best determined and to compare the parallaxes of these stars as obtained by various observatories. One of the great problems in parallax work is to know how to interpret the parallax data of one observatory with respect to data from another. There is no way of knowing *ab initio* how to correct a parallax from Observatory A with that of Observatory B in order to bring all of these numbers onto a common system where there would presumably be no systematic error.

I should also mention that we are getting proper motions out of this

work as we proceed. The solution for the parallax of a star also produces its proper motion, and some very interesting results have already emerged, including the detection of unseen companions. Thus far, perturbations of nine apparently single parallax stars have been established which indicate the existence of binary motion.

The next program I would like to describe is the astrographic catalogue. This is a program which is carried out here at the Observatory and is under the management of Geoffrey Douglass, assisted by Jim Christy and Varkey Kallarakal. Figure 3 shows the twin f:10 eight-inch astrograph used in the program. It is a brand new instrument and consists of two separate telescopes mounted in parallel on a standard 24-inch Boller and Chivens drive. The lens of one telescope is corrected for the blue or photographic spectral band, and the other lens is corrected for the yellow or visual. Each telescope has its own 8x10-inch plate holder and photographs a region in the sky roughly 5x6 degrees. In addition, there is an auxiliary six-inch scope mounted in the cluster which carries a simple right ascension guider, sensitive down to ninth or tenth magnitude. The design of the lenses is rather unique. They are basically of the Ross type, but unlike conventional Ross lenses the filters defining the spectral band passes are permanently mounted in each lens cell as elements of zero power. The hope of this feature is that the filters

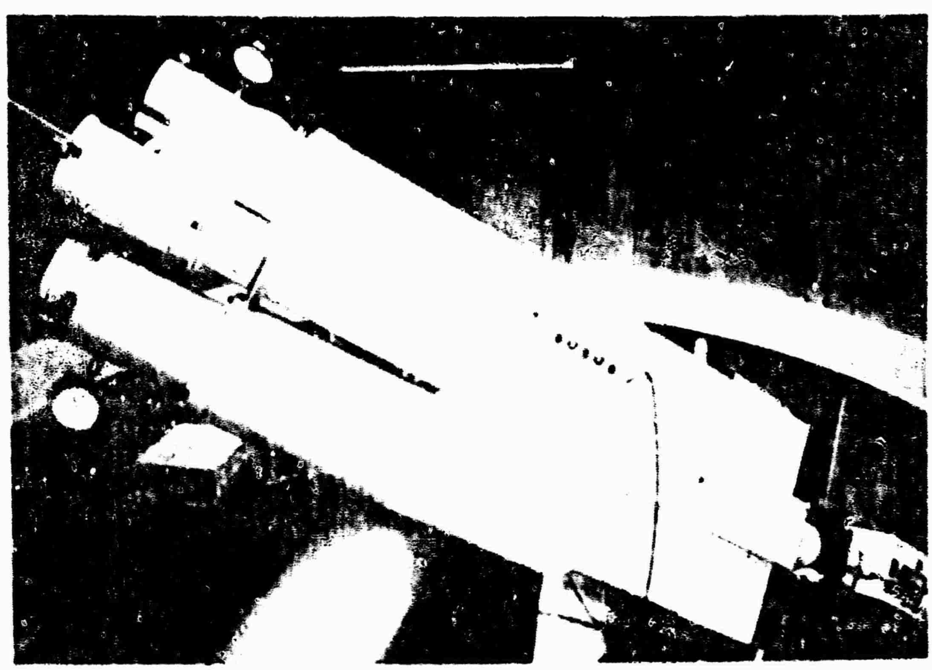


Fig. 3 Twin f:10 eight inch astrograph being used for a new Northern and Southern Hemisphere astrographic catalogue.

will not get broken as they frequently do in other types of astrometric programs, where the filters are placed in plateholders immediately in front of the photographic plate.

Fig. 4. Close-up of the lens area of the eight-inch astrograph, showing the guage that monitors nitrogen pressure.

Another unique feature of these lenses is that they are sealed. There is no way that air from the outside can get into the interior to allow moisture or dirt to condense or deposit over the internal lens surfaces. Now of course you will say, if you have a hermetically sealed lens, how do you handle the pressure problem? This is solved by providing for venting through stopcocks drilled into the walls of each lens cell. All of the inner spaces of the lens cell are connected together by channels, and we can purge the lens with medical quality dry nitrogen from time to time through the stopcocks. Ordinarily the lenses are operated with a slight positive pressure of nitrogen in each lens cell and a sensitive pressure gauge is mounted on each cell for controlling and monitoring purposes (Figure 4). In addition, a bladder is connected to each lens cell by tygon tubing to allow a certain amount of expansion and contraction of the nitrogen gas as the temperature changes. It turns out so far to be a very fine system indeed.

Figure 5 shows Starscan, the new measuring engine used in the astrographic catalogue program. The specifications for this engine were written at the Observatory on the basis of our experience with the SAMM machine. It was fabricated by Optronics International in Chelmford, Massachusetts, and was delivered to us some years back. After a lengthy period of struggle and modification, we finally got all the bugs worked out, and it is now—or was until a lightening surge through the lines fouled up some solid state electronics—working magnificently. A large screen gives a 30 times magnified view of the plate under measurement, which in turn is carried by huge granite stages hidden behind the screen in this photograph. The operator can move the stages around by either handwheels or a joy stick. The stages themselves weigh approximately 1800 pounds and move with practically no friction because they are supported by air bearings.

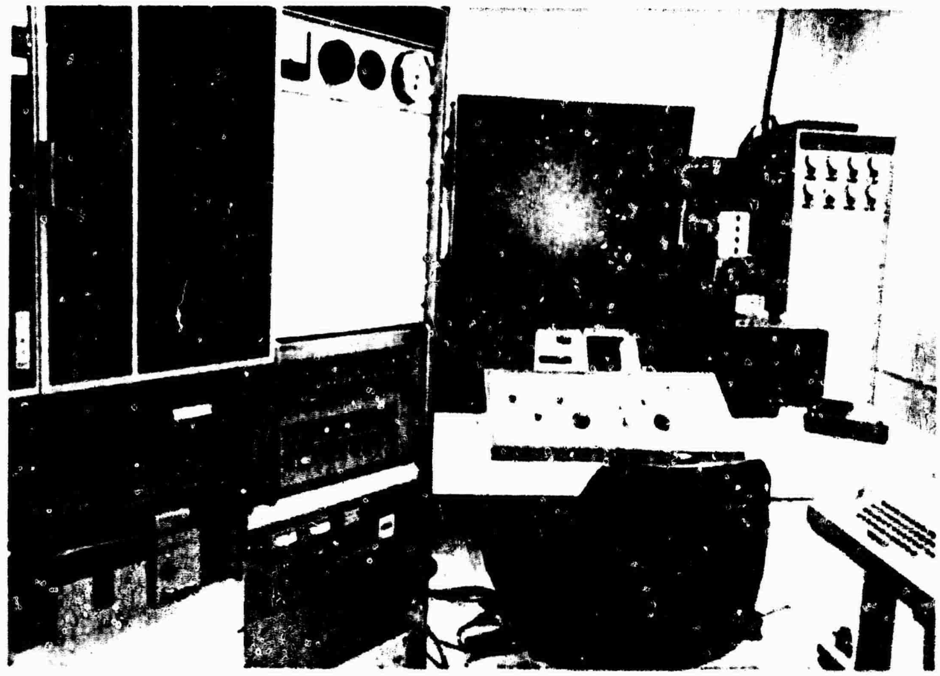
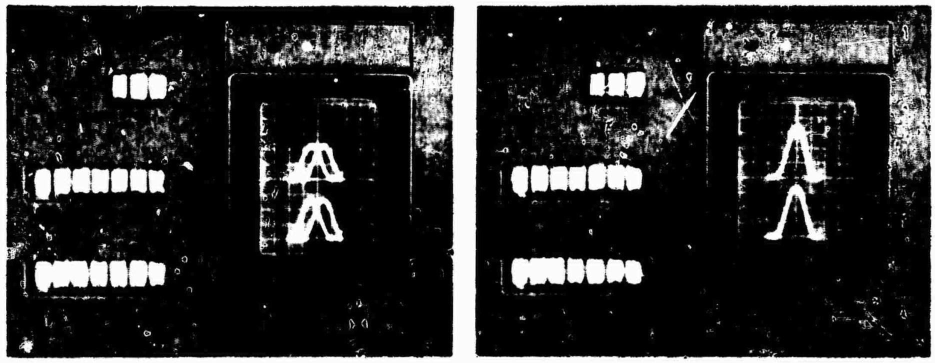


Fig. 5. Starscan, the machine used to measure star positions on the photographic plates of the astrographic program.

Figure 6 shows the real heart of the machine. When a star image is brought close to the optical axis of Starscan, a double Grant density profile of the image is displayed for both the x and y axes on separate small CRTs. When the machine is commanded to center, the double profiles snap together to form a single profile for each axis as shown in Figure 7. Also shown in these figures is the readout of image opacity, which is related to magnitude, and the x and y positions of the image expressed to the nearest micron. These numbers are stored on magnetic tape as the measurements proceed.

Like SAMM, Starscan is a semi-automatic machine. It has the capability to center automatically and to move around the plate automatically, if approximate positional data is fed into the machine to begin with. It is a very fast machine indeed, and has a number of important potentials that we are trying to capitalize on, which I shall discuss in a moment.

The astrographic catalogue program was begun in 1979. We are taking plates now down to the plate limit of magnitude 12 in the visual and 13 in the blue. Our first effort is the zodiacal band, 18° wide, centered on the ecliptic. The plates are being measured down to the limits of the Smithsonian Astrophysical Observatory (SAO) catalogue, namely 9.5 magnitude. We are also remeasuring the original Yale zone plates which were used in the



Figs. 6 & 7. Close-up of Starscan image profiles before (left) and after centering. x, y positions are in microns.

compilation of the SAO. Our initial intention is to publish a zodiacal catalogue as soon as we can which will go as faint as 9.5 magnitude and will give positions to 0.″1 for 250,000 stars. We are using the fundamental reference frame established by the Transit Circle people, basically the ACK3R catalogue and proper motions provided by Tom Corbin.

Progress to date on the astrographic program is a follows: the zodiacal observations are 95% complete, remeasurement of the Yale plates is 70% complete, and measurement of the Naval Observatory plates is 5% complete. The major effort we are mounting at the moment is the upgrading of Starscan itself. We are changing the little on-line HP 2214B computer that came with Starscan to an HP 1000. A program is now being written on contract which will give us the enormous capability of raster scanning plates. We will no longer have to know approximate stellar positions in advance. It will be possible to set the machine up to raster scan a plate automatically and to measure the positions of everything on the plate brighter than a certain predetermined opacity limit.

As to the future, we are going to join the transit circle being sent to New Zealand at the middle of their epoch down there, and we hope to photograph the Southern Hemisphere. Eventually, we intend to publish a catalogue of the Northern and Southern Hemispheres containing something like five million star positions, based upon measurements of twenty million star images covering 8000 astrographic fields.

The next program in EDS is the double star program. The instruments used is the 26-inch refractor (Figure 8), and we have two parallel efforts under way. One is the visual program run by Charles Worley, started in 1961. He uses a standard bi-filar micrometer. The limitations are that the double stars observed usually have separations less than 2″, a magnitude brighter than 12, and a magnitude difference no greater than 5. The accuracy of the

visual observations turns out to be something like 0.″05 in separation as a typical residual.

The photographic work on double stars is done by Jerry Josties, and was begun in 1958. It involves an automatic camera and a series of objective gratings which can be mounted on the 26-inch when the magnitude difference between components is great enough to require it. The magnitude range covered is between +12 and −1, the magnitude of Sirius, and the magnitude difference that can be handled is 8. The accuracy of this sort of work is something like 0.″01 per plate. If many plates are taken over a year and averaged, one can achieve accuracies of about 0.″003-0.″004 for a normal point.

Over 25,000 measures have been made since the visual program was inaugurated. Of these, 60% have a separation less than 1″, which is testimony both to the quality of the astronomical seeing available here at the Washington site and the skill of the observer. The status of the photographic program is that results from 9000 plates have been published.

In addition to observational work, Charles Worley is also responsible for the Index and Observation Catalogues which comprise a master depository of all double star data available in the world today. The Index Catalogue is simply a catalogue of all known double stars, and consists at the moment of 75,000 cards. The Observation Catalogue describes the individual measures for each double star, and at the present time consists of 420,000 measures. This represents complete data from [illegible] present. An effort is presently underway to extend the coverage of the Observation Catalogue back to the first observations of double stars. Plans are to place both catalogues on computer tapes and disks.

Finally, the astrometry of the solar system program was begun in 1946, and uses the 15-inch astrograph here in Washington. Originally 24 minor planets were part of this program to improve corrections to the fundamental reference frame. In addition asteroids with a resonance of 2:1 were observed for a long time to obtain a better mass for Jupiter. Progress to date includes several theses regarding the Jupiter mass problem and major lists of minor planet positions published in 1968 and 1972. Today the data are sent to the IAU minor planet center with selected data to Leningrad and the University of Aarhus. We have now put on the list some additional objects, such as close Earth-passers, asteroids that are likely to result in occultations, and the fainter major planets Uranus and Neptune. We use the 61-inch and the 8-inch astrograph when necessary to augment the results of the observations of the 15-inch. Positions of Uranus and Neptune and positions of the satellites of Jupiter, Saturn, Uranus and Neptune were published in 1979. This is also the program in which the discovery of Pluto's new moon was made by Jim

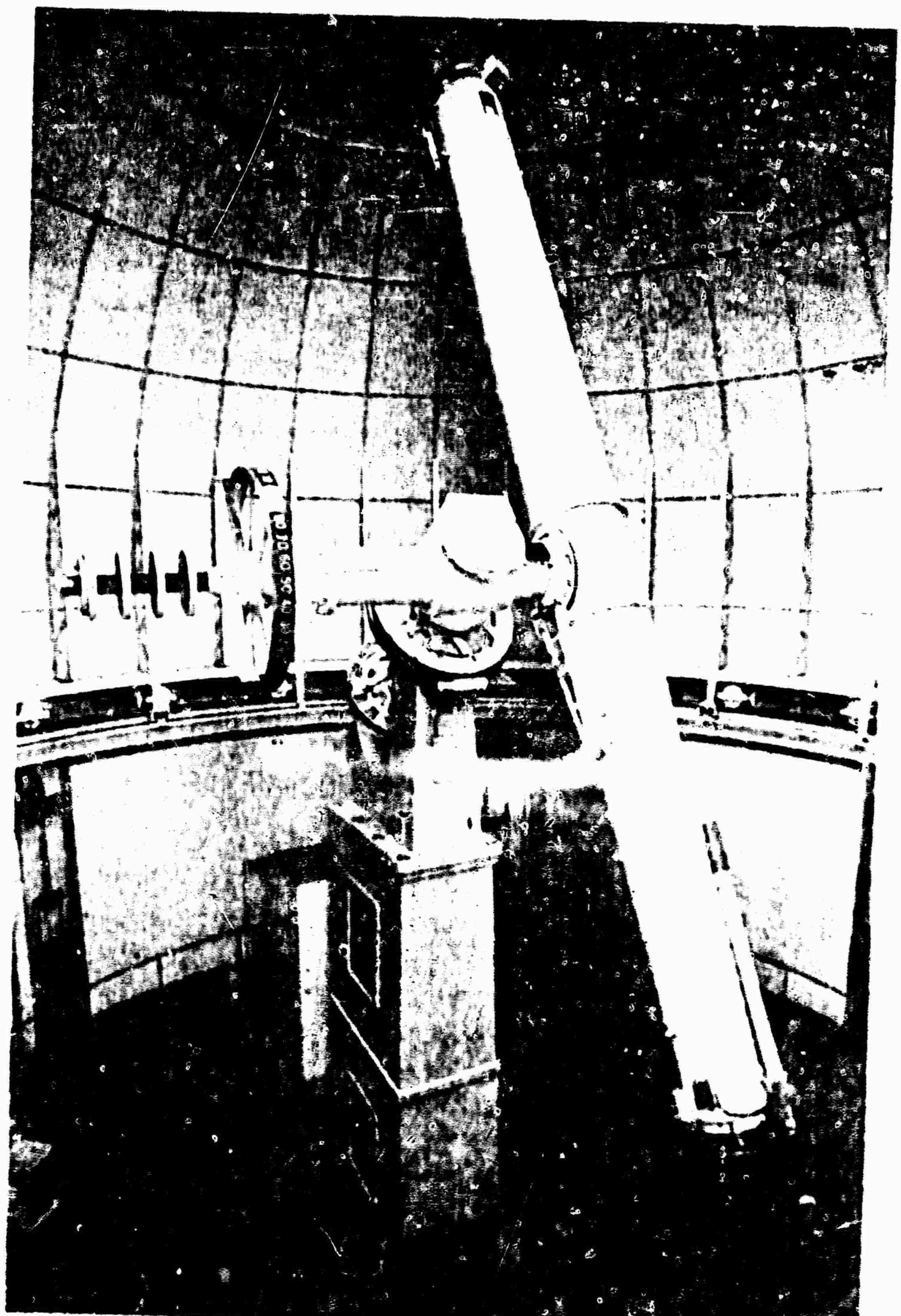


Fig. 8. The 26-inch Alvan Clark refractor, used in the visual and photographic double star program. Asaph Hall used this instrument in 1877 to discover the satellites of Mars. M. Miranian is standing with the telescope.

Christy and analyzed by Bob Harrington in 1978.

This concludes my summary of EDS programs. I hope that I have been able to give you some idea of the range and scope of the work underway and the part EDS plays in helping to fulfill the mission of the Naval Observatory. Thank you for your attention.

N. G. Roman: What are the advantages of the visual double star observations when it appears you can do everything and more photographically?

Routly: The visual observations are restricted to those binaries of 2″ separation and smaller because you cannot observe double stars that close together with photographic plates.

Roman: I understood you to say that the photographic method would separate 1″.

Routly: One second and greater. There is a certain overlap between visual double stars and photographic double stars just to ensure there isn't some kind of a systematic error creeping in between the two techniques.

Q: Is the primary problem with photographic emulsion creep?

Routly: No, it is light scattering in the emulsion. We have not been able to find, no matter how carefully we have looked, any evidence of emulsion creep. If it exists, I think it is at the sub-micron level.

Q: Can you give us an idea of when the final reductions for your zodiacal catalogue will be completed?

Routly: You mean the zodiacal band that we first started out with? We are hoping that this might be ready within a year and a half or two years. The measurements go fairly quickly and the observations are about 95% complete.

Q: On your parallax program, what is your batting average for finding stars that you can measure?

Routly: It was about 20-30% to start with, which was not very good, but we have gotten a lot better. Out of ten possible stars, we now get at least eight or nine stars with measurable parallaxes.

R. T. Clarke (Time Service Division): Thinking long range, you are going ten times farther out with the parallax program. It would seem at some point there would be a need for another telescope. Is anyone thinking of using the 61-inch design, maybe not for something bigger and better, but for something slightly smaller that could nonetheless be useful?

Routly: The main thrust of all astrometric conferences that I have gone to or read about is to get a telescope similar to the 61-inch in the Southern Hemisphere. This is an area in desperate need of attention, just as Dr. Hughes was mentioning in the transit circle work, and I think it is the first priority. The second priority might be to consider a larger telescope that might go fainter than we have. But I doubt whether such a telescope is a serious possibility within the time frame of the next five years. Certainly it would be a wonderful thing to have. More probably I think we will go to CCDs and thereby increase the range, not by building bigger telescopes, but by being much more clever as to how we detect the light.

Clarke: I was thinking that the design of the 61-inch is very good, and even if we had a smaller telescope of similar design, it would still be useful.

Routly: The design is very successful, and people who talk about astrometric reflectors always refer to the 61-inch. That is what I meant earlier by saying it seems to be the standard in the astrometric industry. Certainly smaller telescopes like the 61-inch would be useful.

Roman: If you were building a new telescope, how would you couple the 61-inch with a new approach such as George Gatewood is doing?

Routly: That is a very good question, and there is no immediate answer. We will have to see how successful George is.

Radio interferometer at Green Bank, West Virginia, used by the Naval Observatory to determine Earth rotation data.

CLOSING REMARKS

Gart Westerhout
Scientific Director

While Dr. Routly was talking, it occurred to me that one of the things you see happening here—the parallax program—is a beautiful example of the way in which technological development eventually gets to the point where everything meshes. We developed an astrometric reflector that provided tremendous accuracy for faint star parallaxes. With that experience and the associated expertise, it is now possible to begin measuring a considerable number of bright stars. The whole technology has been developed, and accuracies better than 0".004 are reached without much difficulty. Over the next 10-15 years, a rather major program will be undertaken for getting parallaxes for the important bright stars. That coincides beautifully with the need for parallax data in the 0".01 range which will be required for transit circle and other fundamental star observations in the next 10-15 years. The development of the ability to measure very precise relative positions of objects meshes with the development of the ability to measure very precise absolute positions of objects. I always like the beauty with which these various parts of the work at the Observatory come together.

I would like to end this symposium by taking a look at the future (perhaps, some will say, through rose-colored glasses) by means of Table 1. The program outlined in this table shows the need for research as we see it at this time. A program like this would cost on the order of five or six million dollars per year and would be undertaken by a multitude of organizations: the Naval Observatory, other Navy and DoD laboratories, universities and industry. Some of it is funded currently at a low level. In the following, I will address each line in Table 1 in succession.

a. We must continue the work that we are doing in Flagstaff and here in Washington on positions, parallaxes, motions, colors, radio stars and double stars, all of which are measurements in direct support of the fundamental work associated with the almanacs, time and fundamental star positions.

b. A project that Dr. Hughes didn't mention, but on which we have started, is development of a new fundamental reference frame from scratch: that is, one not built upon previous fundamental catalogues. In other words,

Table 1. *U. S. Naval Observatory R&D Program*

a. Positions, Motions, Colors of Special Objects, Direct R&D Mission Support
b. New Fundamental Reference Frame
c. Celestial Mechanics (Orbit Theory)
d. Green Bank Interferometer
e. Earth Rotation Studies
f. Atmospheric Studies
g. Laser Time Transfer (Project LASSO)
h. Master Clock Upgrade. Basic Clock Development
i. Optical and Infrared Interferometry
k. Large Angle Measurement Techniques (Ground Based and Satellite)
l. VLBI Time Transfer
m. Laser Time Transfer and Ranging (Expanded Capability, incl. Moon)
n. Photographic Astrometry
o. GPS Time Receiver Development
p. Navigation Systems

this would not be an improved FK5, but a new Naval Observatory fundamental reference frame, which, because of the nature of the way in which it would be handled, would be independent of the FK5. Our New Zealand effort is essential in this undertaking.

c. In the area of celestial mechanics, Dr. Seidelmann showed that there remains a considerable amount to be done on orbital theory of the motions of the planets. This work can take many years and can be done both here at the Observatory and in the academic community.

d. The Green Bank interferometer is producing extremely interesting results with such accuracy that we feel that these, combined with the results from the photographic zenith tubes and the Navy transit satellites, provide three-day averages of Earth rotation data which are at least of the same accuracy as the five-day numbers we get from the Bureau International de l'Heure. But there is still a lot to be learned, particularly about atmospheric influences on the precise determination of the positions of radio sources and thus on the determination of Earth rotation. Several more years of studies are needed to make the Green Bank interferometer a completely operational instrument to which no further improvements can be made.

e. With all the new data coming in every three days, giving very much more precise and timely information about the wobble and the rotation of the Earth, we might now be able to go back and study the causes of the variations in the Earth's rotation. Some geophysicists are very much interested in taking those results and going on with further studies in that direction.

f. I have already mentioned atmospheric studies. Dr. Hughes mentioned the start we are making on atmospheric studies in order to finally come to grips with the last one-half of one percent of the refraction corrections in any one of the various methods with which one can measure stellar positions from the surface of the Earth. We simply must get that problem under control, because it is really at present the limiting factor in getting greater accuracies.

g. In the area of time transfer we are, as Dr. Winkler mentioned briefly, participating in an experiment in which time will be transferred from here to Europe and back using a satellite and laser ranging. This is a European project in which the Naval Observatory is the American partner. With the collaboration of NASA-Goddard, we are using the Goddard 48-inch telescope; Dr. Alley and his group at the University of Maryland are the main participants in the experiment.

h. Upgrading the Master Clock, as Dr. Winkler mentioned, involves the acquisition of hydrogen masers within the next five years. Basic clock development will further increase our capabilities in the long range.

i. Regarding the fundamental reference frame, greater accuracy in stellar and planetary as well as lunar and solar positions requires the development of large-angle measurement techniques more accurate than the divided circle. There is hope for very considerable progress in this development over the next 5-8 years. An entirely different method of measuring large angles is optical and infrared interferometry, which shows great promise.

j. In all these areas, as well as in the area of differential measurements of positions, such as parallaxes, the Observatory is looking very hard at methods to replace, at least partially, the photographic plate. This is in order to get better results, not because we hate photographic plates. We are simply trying to pick out those areas in which such things as array detectors, e.g., charge coupled devices (CCD's), might eventually enable us to get results that are more accurate than one can derive with the combination of photographic plate and measuring machine. Of course, a catalogue of five million stars, for which twenty million images are measured, is entirely impossible to do in the foreseeable future using photoelectric means. Therefore, the future of large-scale astrographic programs, such as the one Dr. Routly described, requires, for a long time to come, the availability of extremely powerful measuring machines for photographic plates. At the same time, light detectors and automatic position measuring devices in the focal plane, in which Dr. Hughes is pioneering in the Transit Circle Division and which we are involved in at Flagstaff, are beginning to make some small inroads. Through collaboration with various universities, I think this will come to the fore in

the next five years or so.

k. Dr. Hughes showed a slide of the possible development of an astrometric satellite, and I would like to briefly show you the sort of time scale that one has in mind when one talks about a capability in space astrometry. Figure 1 assumes that we would start this in fiscal year 1982. We would estimate a period of about three years to look at the subsystems, such as large-angle measuring devices, new gyro systems, focal plane mensuration devices and so forth. A period of two or three years would be required to develop these, test them out, and implement them on ground-based equipment. Virtually everything that one develops for an astrometric satellite is going to improve ground-based observation, particularly if one has gone a step further in measuring atmospheric refraction. Eventually, in a period of five or six years, this development might lead to the first proposal for a satellite, with a two-year design study and a final satellite proposal. This takes us to the year 1995, on a more or less optimistic scale. Just to indicate what such a development would look like for the first five years, I could conceive of us working on large-angle measurement techniques with such an organization as Draper Laboratory, while at the sime time tackling the entirely different concept of optical and infrared interferometry. It is by no means certain—that is why one talks about a five year development stage before one even makes a proposal for a satellite—that an astrometric satellite will look like a transit circle. At the same time, whatever one does in developing such instrumentation, one must always remember that many of the requirements come from systems located on the surface of the Earth, and therefore atmospheric studies, even if one eventually has a satellite, are still important. I have already mentioned focal plane mensuration devices in this regard. All this goes to show that a 15-year development of a major new approach to

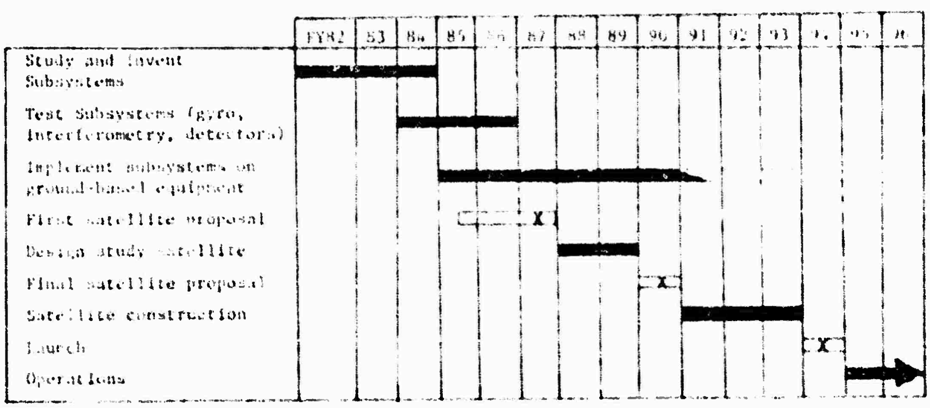


Fig. 1. Schedule for development of capability in space astrometry.

astrometry will at the same time, as a by-product, very thoroughly improve the many areas in astrometry in which we are now working. Even if a satellite never comes to the actual funding stage, such intermediate research and development will have very considerable influence on our further work.

l. I have mentioned earlier the use of lasers for time transfer: VLBI is another very powerful means of time transfer, i.e., synchronization of clocks. The Naval Research Laboratory and the Naval Observatory are heavily involved in the first tests of this method.

m. At the same time one could conceive of an expanded capability in laser time transfer and ranging, following the example of such institutions as the Royal Greenwich Observatory and several others in the world of astrometry, to establish an actual laser ranging system. Such a system would not only be used for time transfer, but also for ranging to satellites and even to the Moon to allow ever more accurate studies of perturbations in the lunar orbit, as well as better determinations of the Earth's rotation.

n. In the area of photographic astrometry, I have already mentioned that we will have the photographic plate for a very long time. Therefore, it is quite clear that further development and study of better measuring machines is still of foremost interest at the Observatory.

o. In the area of time transfer the GPS is one of the satellite systems that can provide, at a somewhat lesser accuracy than the laser or VLBI methods, time transfer capability for the user. The Naval Observatory, because of its DoD mission as the coordinator of precise time and time interval, is the organization which should monitor and push for the development of time transfer receivers for the uninitiated user: You turn it on and have the time to 20 nanosecond accuracy, just like that. That is the kind of thing that we would eventually like to give to every soldier in the field, just for setting his watch.

p. Finally, as part of a task we have recently undertaken—improving navigational training in the Fleet—it is quite possible that we will be looking into development projects for navigation systems. Such systems will perhaps integrate automated sextants with the various radio navigation aids into one system, whereby the user can utilize whatever he has, with many fall-back positions. This is an area which is typically related to our mission, but which disappeared from the Naval Observatory a long time ago.

With this grandiose scheme for all the things we can do in front of us, it seems to me that the Naval Observatory, if given the wherewithal, is an organization that has an absolutely marvelous future. We will see you again in 150 years.

APPENDICES

HOW THE U. S. NAVAL OBSERVATORY BEGAN, 1830-65*

Steven J. Dick
U. S. Naval Observatory

Exactly a century and a half ago, in Washington, D. C., the Navy set up the Depot of Charts and Instruments, forerunner of the U. S. Naval Observatory. It was approved by John Branch, Secretary of the Navy, on December 4, 1830; two days later orders from the Board of Navy Commissioners implemented its formation.

These events initiated an important chapter in American astronomical history, leading to the establishment of the earliest large observatory in the United States, comparable to the famous Greenwich and Paris observatories founded 150 years earlier. Just how a small depot for naval instruments was transformed into a major observatory is a story that has remained obscure even to historians of astronomy. Here are the details, reconstructed from naval, scientific, and biographical documents in the National Archives, Library of Congress, and Naval Observatory library.

Early Attempts to Form a National Observatory

Unlike astronomy in Britain and France, in 1830 the science in America was still in its infancy. A few individuals — David Rittenhouse, John Winthrop, Benjamin Banneker and Nathaniel Bowditch — had made appreciable contributions. But the benefits of systematically observing the heavens were by no means obvious to the populace nor to their representatives in Congress. At least two earlier calls for a national observatory had failed, and a third attempt under consideration was similarly doomed.

The first stemmed from legislation sponsored by Thomas Jefferson in 1807 to survey the American coastline. Swiss-born mathematician and surveyor Ferdinand Rudolph Hassler, whom Jefferson had appointed to head the new Coast Survey, recommended establishment of two astronomical observatories. But legislation repealed the Coast Survey's authorization in 1818, before Hassler could set up the two five foot long astronomical transit telescopes he had obtained from Eiward Troughton in England. The second

*Reprinted by permission of Sky Publishing Corporation from *Sky and Telescope*, vol. 60, no. 6, December 1980, with corrections and notes added.

attempt at a national observatory began on December 15, 1809, when amateur astronomer and mathematician William Lambert sent a report to Congress calling for the establishment of a prime meridian in the United States. Finally receiving approval by a joint resolution of Congress on March 3, 1821, Lambert set up a temporary observatory in the south wing of the Capitol building itself and used some of Hassler's instruments. They were taken down later that year, when his work was completed.

No less a figure than John Quincy Adams made the third call for a national observatory. His first annual message as President, dated December 6, 1825, asked for an institution and "support of an astronomer to be in constant attendance of observation upon the phenomena of the heavens."

Unfortunately for President Adams and American astronomy, congressional opposition, fanned by partisan politics, was so great that when the Coast Survey was re-established in 1832, Congress specifically admonished that nothing in the act authorized construction of a permanent astronomical observatory. Adams' repeated appeals, almost until his death in 1848, were to no avail.

The Depot of Charts and Instruments

Despite opposition from Congress, a modest chain of events in the Navy Department would lead to success where all else had failed. Not surprisingly, Navy interest sprang from the practical needs of navigation. As early as November 1829, the Board of Navy Commissioners (three select officers who assisted the Navy secretary) pointed out the need for greater care in the purchase and regulation of ships' chronometers.

These were the important timepieces by which ships at sea maintained accurate port time. By comparing that to local time, as derived from sextant observations of the Sun, Moon or stars, navigators could determine longitude. John Harrison in England invented the practical chronometer in 1759, but the demand for these delicate clocks long exceeded the supply, and they came into widespread use in the American Navy only in the 1820's.

Probably alerted to the Navy Commissioners' report by his father, the influential Secretary of the Board, in November 1830, Lt. Louis M. Goldsborough submitted to the Secretary of the Navy an eloquent and persuasive paper about a chronometer depot. At age 26, the lieutenant was already a navy veteran of 15 years sea duty. He emphasized that the lack of systematic purchasing and care had resulted in exorbitant prices for inferior chronometers. Eight out of 10 naval ships set to sea without an accurate determination of how much their chronometers ran fast or slow. An error of only four minutes of time during a long voyage could lead to a fatal mistake of a full

Lt. Louis M. Goldsborough, founder and officer-in-charge of the Depot of Charts and Instruments, 1830-1833.

Lt. Charles Wilkes, officer-in-charge of the Depot of Charts and Instruments, 1833-1838.

degree of longitude.

Goldsborough, therefore, suggested that all naval instruments and charts be kept in a depot, where a competent officer could "rate" the chronometers. This rate—the amount of deviation from true time, due to temperature changes, mechanical imperfections, and so on—could then be taken into account when determining longitude. For accurate rating, the depot had to make regular astronomical observations, since no clock or other periodic phenomenon known was more accurate than the time given by a celestial body's passage across the meridian, determined by a transit instrument.

On December 6, 1830, the Board ordered Lt. Goldsborough, then in Washington, to "proceed forthwith to Philadelphia, New York, Boston, Portsmouth, N. H., and Norfolk, Va., where you will receive from the respective commandants, all the chronometers, sextants, theodolites, circles, and nautical instruments of value, and not in use—all of which are to be transported to this place."[1] This provided the core for the new depot.

Goldsborough rented a house on G Street, between 17th and 18th Streets, in the northwest quadrant of the city, near the White House and the Navy Department. He began the necessary celestial observations for rating chronometers, first with a sextant and circle, but between 1831 and 1833 with a transit instrument. It was built in New York by Richard Patten and had a 30-inch long tube. The board believed this to be "the only one in the United States for sale."[2]

This transit was placed in a small circular building and mounted upon a brick pier whose base extended 20 feet below ground level. Thus, practically from its founding, the Depot of Charts and Instruments consisted of the actual depot plus a small observatory where observations were made.

Goldsborough not only rated the chronometers, he also had to deliver them (with other charts and instruments) to ships, and bring back others to the depot for rating and repair. He complained that this burden forced him to suspend "the duty of making observations, which should be constantly attended to every fair day and every clear night."[3] The Navy, therefore, supplied him with a passed midshipman (one who had passed a specified set of examinations) to assist with the duties. Thus, from July 1831, there began a long tradition of passed midshipmen assistants at the Depot.

Goldsborough's duties, and the Depot itself, remained much the same until he departed on February 11, 1833, for the war with the Seminole Indians in Florida. When he left the Depot the inventory showed healthy growth, with a wide variety of meteorological and magnetic, as well as astronomical, instruments.

The Capitol Hill Observatory

Impetuous and flamboyant Lt. Charles Wilkes, then surveying Narragansett Bay, was put in charge of the Depot on March 12, 1833. He became a midshipman in 1818, served in the Mediterranean and South Pacific, and was promoted to lieutenant in 1826. Wilkes is best remembered as commander of the exploring expedition to the Pacific (1838-42) and for writing his detailed accounts about it.

Wilkes found the G Street site too "vaporous" for astronomical observations. It was probably too close to the Goldsboroughs, father and son, as well, and Wilkes preferred to have the depot near his own residence to enable him "to make at all hours of the day and night the necessary observations to the best possible advantage."[4]

Thus, in May, 1833, he moved it to a site 1,200 feet northwest of the center of the Capitol, where he had bought several houses built by George Washington. The upper floor of one served as the new Depot. The Navy granted permission for the move only reluctantly and asked Wilkes to bear the entire expense—a decision the Navy came to regret.

The following winter, Wilkes built, again at his own expense, a new observatory for the transit instrument. It became known informally as Capitol Hill Observatory.

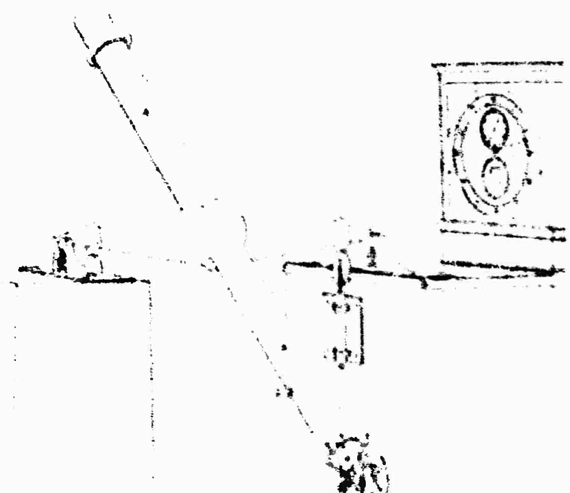
Its essential features were described by James Melville Gilliss, Wilkes' successor:

> The length of the observatory, east and west, was fourteen, its breadth thirteen, and height from the floor to the eaves, inside the plastering, ten feet—its roof sloping to the north and south at the usual inclination in buildings covered with wood. Meridian doors, each nearly two feet wide, and making, together, an aperture more than three and a half feet, reached from the floor to the eaves: they opened outwardly, and were lined inside with baize. The roof doors were of the same width, but extended only to within three feet of the ridge pole. These were raised by pulleys leading over uprights on the roof, through sheaves in the east and west sides of the building. There was a door of ordinary dimensions in each of these sides; and to prevent the transmission of terrestrial vibrations, the building was surrounded by a ditch five feet wide and deep.[5]

Because the two sides of the roof were held together at the zenith, 12 of the 88 standard stars contained in the British Nautical Almanac could not be observed, even after the building was modified in 1838.

Wilkes broke with the past also by retiring the Patten transit and using instead one of the brass Troughton transits Hassler had bought for the Coast Survey in 1815. It was 63 inches long with a clear aperture of 3.75 inches and was supported on massive granite piers 10 feet long. An observation consisted in noting, with reference to an astronomical clock, the exact time an object crossed each of five vertical wires set in the eyepiece's focus.

Wilkes also set up a magnetic observatory 50 feet south of the transit



The 3.75-inch Troughton transit had a five-foot-long brass tube. The fine graduated semicircles flanking the eyepiece served as finders in declination.

building and 40 feet northwest of the house occupied by the depot. Thus, the Depot of Charts and Instruments involved three separate buildings.

Prior to 1838, Gilliss later noted, "the transit instrument appears to have been mainly employed in observing the Sun, to determine the errors of the clock with which the navy chronometers were daily compared, and to this end, the time was noted by one of the many chronometers under rate. Observations by night were rare; and eclipses, occultations or other astronomical phenomena, except that of the Sun in 1836, seem to have received little attention."[6] Wilkes' only surviving observing book notes 78 transits of the Sun but only seven of stars between January and November 1836, when Gilliss arrived as a passed midshipman.

With Wilkes' departure in August 1838 for his exploring expedition, Gilliss' talents blossomed. To determine longitude differences between Washington and points Wilkes would visit, the Secretary of the Navy ordered Gilliss to observe stars day and night. Eclipses of the Sun, Moon and Jupiter's satellites, and other interesting astronomical phenomena were also to be observed as they occurred. William C. Bond in Dorchester, Massachusetts, made corresponding observations.

Gilliss had entered the Navy in 1826 at the age of 15 and studied briefly at the University of Virginia. He was largely self-taught in astronomy, basing his studies on two books of the era.

He wangled some new instruments to satisfy the demand for more observations, such as a portable 3¼-inch refractor of 42-inch focal length, a variation transit, a dip circle, and a sidereal chronometer. From November 21, 1838, to the expedition's return in June, 1842, Gilliss himself observed more than 10,000 transits of the Moon, planets and stars, and each year averaged 110 culminations of the Moon and 20 lunar occultations. In 1846 Gilliss published the right ascensions of 1,248 stars in *Astronomical Observations Made at the Naval Observatory*. It was the first star catalogue based on American observations, even though its declinations were borrowed from the *Catalogue of the Royal Astronomical Society*.

A Permanent Naval Observatory

The events of 1842 formed a second watershed in the growth of the observatory. Since the previous September, Gilliss had agitated for a new building for the transit. The Board and the Secretary of the Navy recommended to the President that a new one be erected, and in March a bill was introduced in Congress. The Wilkes observatory was deteriorating, as is evident from Gilliss' politely worded letter of February 15, 1842: "I have the honor to report, that the doors of the observatory attached to the Depot were

blown down yesterday again, being the third time this winter. The extremely valuable instruments it contains were thus entirely exposed to the weather."[7]

Neither the Navy nor Congress decided to abandon the old observatory, however. Wilkes, who returned in June, charged that proper care had not been taken of his property and ordered the Navy to vacate by the end of the month. This was quite a bold demand from an officer still early in his career, but typical of the man who later almost precipitated a war with England by forcibly removing Confederate commissioners from a British ship (the Trent affair). Left with no choice, the Navy moved the Depot to temporary quarters on Pennsylvania Avenue.

Gilliss resigned from the Depot on July 7, 1842, and with the fledgling institution relegated to temporary quarters its future might have been in doubt had it not already proven its worth. The Navy acted quickly, and appointed Lt. Matthew F. Maury on July 12 to take charge. Ironically, it was Maury who had scathingly attacked the inefficiency of the Board of Navy Commissioners; a few months later it was replaced by five bureaus. The Depot came under the Bureau of Ordnance and Hydrography, where it received more adequate attention to its growing needs.

Congress was starting to pay attention, too. Gilliss had sighted Encke's comet with the 3½-foot-long refractor, and the publicity helped Congress to pass—in the final hour of its 1841-42 session—a bill providing $25,000 to

Lt. James Melville Gilliss, successor to Lt. Wilkes at the Depot, 1838-1842. Superintendent of the Naval Observatory, 1861-1865.

Lt. Matthew F. Maury, successor to Lt. Gilliss at the Depot, 1842-1844, first Superintendent of the Observatory, 1844-1861.

erect a new Depot. That same session had turned Adams down again, this time to apply part of James Smithson's bequest to a national observatory, but it did approve funds for an observatory under the name of the Depot of Charts and Instruments.

Gilliss never doubted that the new Depot had to be much more than a storeroom. He recalled in 1845, "I should have regarded it as time misspent to labor so earnestly only to establish a depot. My aim was higher. It was to place an institution under the management of naval officers, where, in the practical pursuit of the highest known branch of science, they would compel an acknowledgement of abilities hitherto withheld from the service."[8]

Gilliss had been ordered, immediately upon his resignation, to reduce his transit observations. Now, however, he was sent to the major cities of the north to consult with prominent scientists: Alexander Bache, W. H. Bartlett, William C. Bond, Ferdinand R. Hassler, R. T. Paine, R. M. Patterson and Sears C. Walker. Each suggested features for the new observatory. With plans in hand, Gilliss sailed for Europe to consult further with astronomers there and to purchase instruments and books.

Returning in March 1843, Gilliss personally supervised construction on an elevated site chosen by President Tyler. "University Square" had been proposed by George Washington for the site of a national university; it was west of the White House and close to the Potomac River. It took 1½ years to build the 50-foot-square brick central edifice and the wings to the east, west and south. The central part included offices, the depot, and a library. A revolving dome 23 feet in diameter stood atop this structure. Each wing contained meridian instruments.

Gilliss reported completion of the observatory in the fall of 1844. To his utter dimay, it was not he but Maury who was named Superintendent on October 1. In his private correspondence Gilliss made no secret of his feelings:

> Although there is no duty so agreeable to me as astronomical labours, yet, I have too much pride to solicit a connection with an establishment whose existence is owing solely to myself. You know, the law recognizes it only as a Depot for the Charts, etc., of the Navy, and Lt. Maury has moved into it, with his instruments, charts, etc. What the intention is, I know not, for I have also too much pride to enquire . . . If it is to be an observatory Maury is not the man to be at its head, unless he has an entirely different taste from that induced by his previous life and labours.[9]

Maury was not an entirely unlikely choice to head the Depot, however. Though he had little formal training in astronomy, he had circumnavigated the globe, learning practical navigation well enough to write in 1836 *A New Theoretical and Practical Treatise on Navigation.* When an accident in 1839 left him lame and confined to shore duty, he spent much of his time writing articles on the reorganization of the Navy. He took charge of the Depot at its temporary quarters in 1842 and lived there with his family.

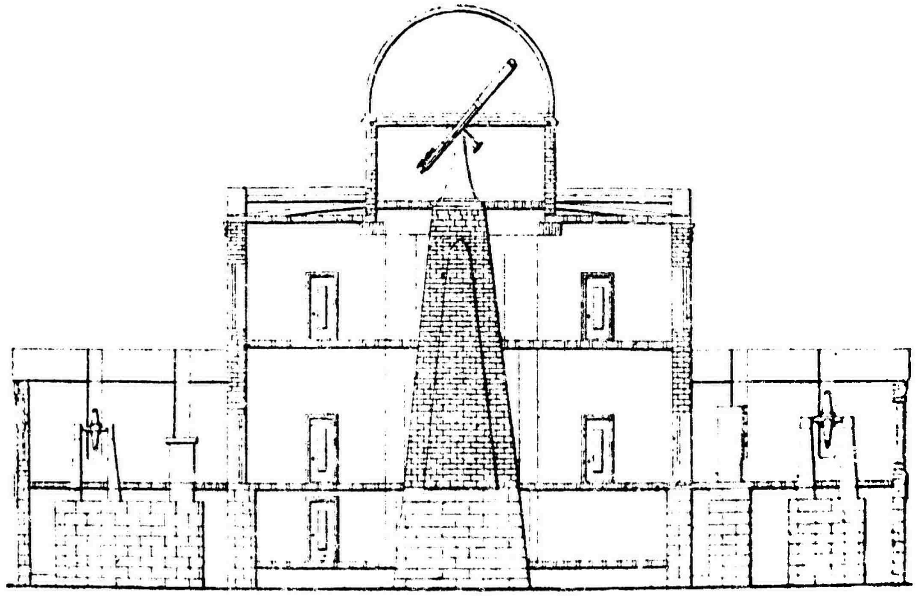
Maury set to work with a diligence that might have surprised even Gilliss. He now had the greatest collection of astronomical instruments yet assembled in America. Gilliss described them in this way in 1845:

> 9.6-inch achromatic refractor, focal length 15 feet, 3 inches, by Merz and Mahler. It cost $6,000, of which $3,000 was for the objective lens.
>
> 5.5-inch transit instrument, focal length 88 inches. The Merz and Mahler objective cost $320; the instrument was built by Ertel and Sons for $1,480.
>
> 4-inch mural circle, 5-foot focal length, by Troughton and Simms, costing $3,550.
>
> 5-inch transit in the prime vertical, 78 inches in focal length, made for $1,750 by Pistor and Martins.
>
> 3.9-inch comet seeker, just 32 inches in focal length, by Merz and Mahler, for $280.

Another transit instrument, and a transit circle, also came to the Depot about that time.

Thus, instead of the single meridian instrument Gilliss had used until 1842, the new observatory suddenly possessed five of them, and a very large refractor. Since each telescope had different capabilities, there was no redundancy. The transit instruments only determined right ascensions, by precisely timing stars' passages over the meridian. The mural circle, a peculiarly English instrument, could only determine declinations. The transit circle (meridian circle) and prime vertical could find positions in both coordinates, using different methods. By acquiring both a mural circle and a prime vertical, Gilliss could compare the accuracies of the two instruments, which represented the English and German schools of positional astronomy.

Duties expanded, too. The officers and passed midshipmen made meteorological observations regularly, collected hydrographic data, constructed charts, tried and rated chronometers, and bought all nautical books, maps, charts and instruments the Navy required. And within a year of his appoint



East-west cross section of the new observatory, from Gilliss' 1845 "Report of the plan and construction of the depot of charts and instruments . . ."

ment, Maury had the prime vertical, the mural circle, and the transit instrument operating.

From 1845 on, the observatory made regular meridian observations of the Sun, Moon and planets. Maury even proposed in 1846 that a star catalogue be prepared for use in a nautical almanac, embracing all stars visible with the instruments. The Navy approved, and Maury began the work.

The time service, previously limited to rating chronometers, added a time ball. This was a black canavas structure, 2½ feet wide, dropped from the mast atop the dome every day precisely at noon. The time ball mast is prominent in the earliest sketches of the observatory.

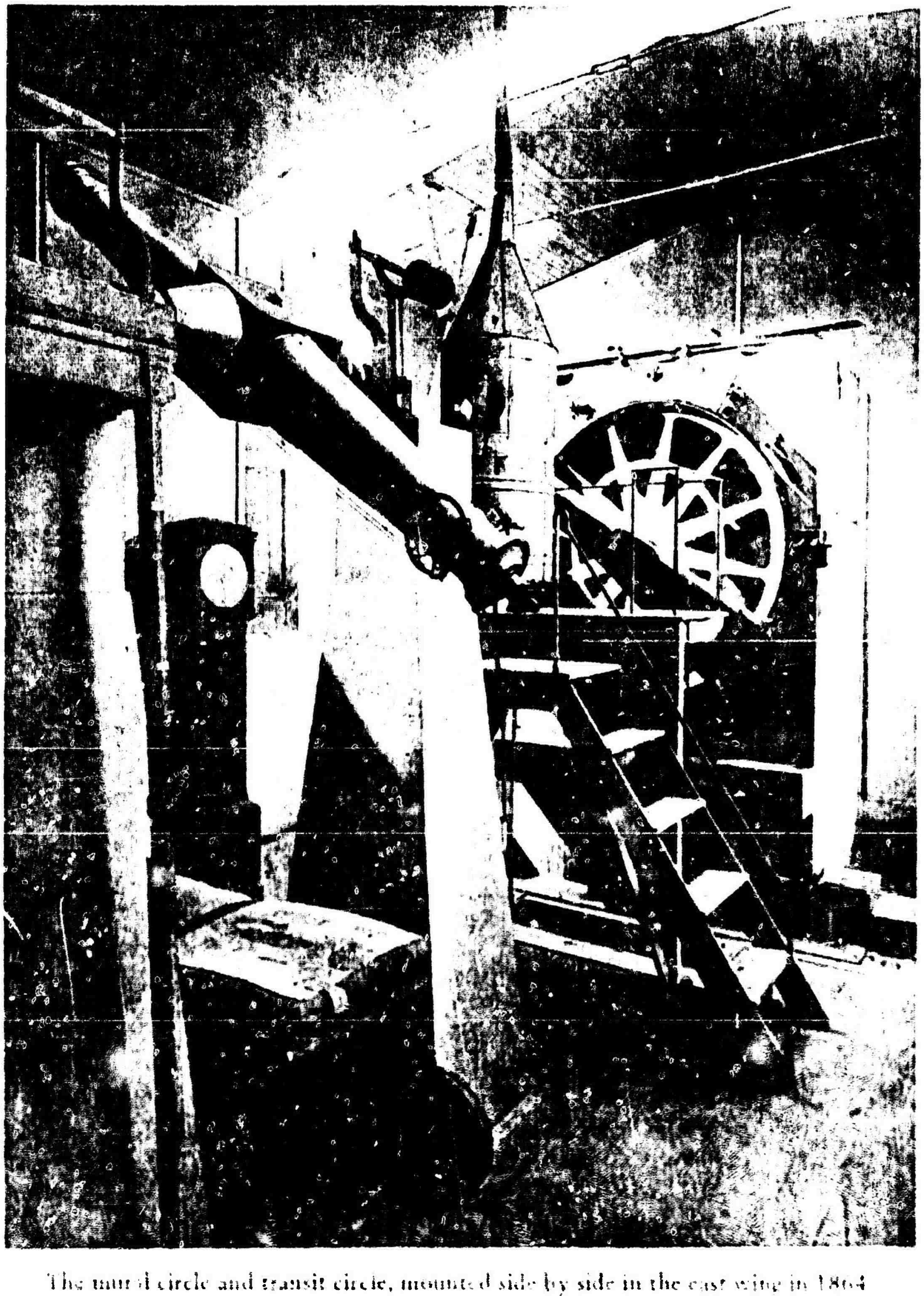
The staff grew from six passed midshipmen, three lieutenants, and a machinist to include another three lieutenants and members of the Corps of Mathematicians, which was previously associated with the Naval Academy. The ranks of Navy professors who joined the Observatory over the next two decades included John C. Coffin (1843), Joseph S. Hubbard (1845), Ruel Keith (1847), M. Yarnall (1851) and Simon Newcomb (1861). Sears C. Walker, a civilian, arrived from the Philadelphia High School Observatory in 1846 but returned there after 1½ years because of conflicts with Maury. James Ferguson, also a civilian, joined the staff in 1848 and observed with the great refractor until 1867.

The Merz and Mahler 9.6-inch refractor, mounted in 1844.

The "first volume of Astronomical Observations that has ever been issued from an institution properly entitled to the name of Observatory on this side of the Atlantic," as Maury termed it, was published in 1846.[10] It included prime-vertical, transit-instrument, and mural-circle observations, and culminated with a catalogue of 98 stars whose right ascensions and declinations had been precisely determined.

This volume established the international reputation of the observatory. Further attention came in 1847 when Walker discovered that Neptune had been observed by J. Lalande in 1795, long before its discovery, thus enabling a more precise determination of its orbit.

Technical improvements included the introduction in 1849 of a chronograph for automatically recording transit times of stars, thus replacing the laborious and less accurate eye and ear method. By the end of the 1840's the observatory was a regular part of life in Washington and the nation.



The mural circle and transit circle, mounted side by side in the east wing in 1864. The mural circle provided only declinations, the transit only right ascensions.

and was known and respected around the world.

It was also becoming evident that Maury's program was too ambitious. By 1849, 38,000 observations of stars remained to be reduced, so the herculean program of producing an all-encompassing star catalogue had to be abandoned. The war against Mexico seriously depleted Maury's junior staff. When forced to choose which duties to maintain, Maury turned to hydrography rather than astronomy.

Though Yarnall kept observing about 500 stars per year, from 1849 to 1860 Maury and the rest of his staff produced famous wind and current charts, which required the collection and coordination of data from ships all over the world. Maury's interpretations of these data, and his unorthodox theories in other areas, made him something of an outcast among scientists, but his work and the textbook *The Physical Geography of the Sea* (1855) justly earned him the title of "father of oceanography."

Though the 1850's at the Naval Observatory belonged to oceanography rather than astronomy, Ferguson continued observations with the refractor. He discovered three minor planets: 31 Euphrosyne in 1854 (the first found from America), 50 Virginia and 60 Echo. Astronomical observations were published sporadically, and Yarnall's did not come out until 1872. At first these appeared under the name "National Observatory" but after 1853 "U.S. Naval Observatory," anticipating the Secretary of the Navy's order of December 12, 1854, designating the institution "The United States Naval Observatory and Hydrographic Office," to recognize how the Depot had grown and that the Navy was in charge.

When the Civil War started in the spring of 1861, Maury, who was born in Virginia and had lived in Tennessee, resigned to join the Confederacy. He left Washington amid charges of treason. None other than Gilliss was appointed to take his place, finally gaining the position he had hoped for 16 years before. During the interval he had reduced and readied for press his transit observations from 1838 to 1842, initiated and headed a Navy expedition to Chile to determine solar parallax, and led solar eclipse expeditions to Peru and Washington Territory. He spent four years preparing a four volume account of the U. S. Naval Astronomical Expedition, 1849-52.

Benjamin Apthorp Gould, a prominent astronomer who was apparently no fan of Maury, wrote of Gilliss' 1861 appointment: "The sudden transformation which took place was like the touch of an enchanter's wand. Order sprang from chaos, system from confusion, and the hearts of the faithful few who had struggled on for years, hoping against hope, were filled with sudden joy."

The Civil War burdened the Observatory with providing instru[illegible]

charts and chronometers for a greatly enlarged navy. Still, Gilliss managed to publish old observations that Maury had left unreduced, and undertook about 2,500 star observations each year. Five volumes of observations were published under Gilliss' direction, as many in five years as in Maury's 19. In addition to Newcomb, in 1862 he appointed Asaph Hall, William Harkness and J. R. Eastman to the positions of assistant astronomer. Astronomy became vigorous once more. A new 8.52-inch transit circle was mounted in 1865.

Gilliss died suddenly in February 1865, perhaps from malaria contracted from the swampy surroundings of the Observatory. In a scene reminiscent of Copernicus, he received on the morning of his death the published observations of solar parallax made from observatories in Washington and Chile that he helped found.

The Hydrographic Office was separated from the Naval Observatory in 1866, and the Nautical Almanac Office moved to Washington from Cambridge, Massachusetts, that same year. It became an official part of the Observatory in 1893, when that institution itself moved to its present location on Massachusetts Avenue (see *Sky and Telescope*, December 1951, page 27).

With the arrival of Newcomb, whose name is so closely linked with the 26-inch refractor that came in 1873 (see *Sky and Telescope*, October 1973, page 208), and Hall, discoverer of the moons of Mars, the history of the Observatory becomes more familiar.

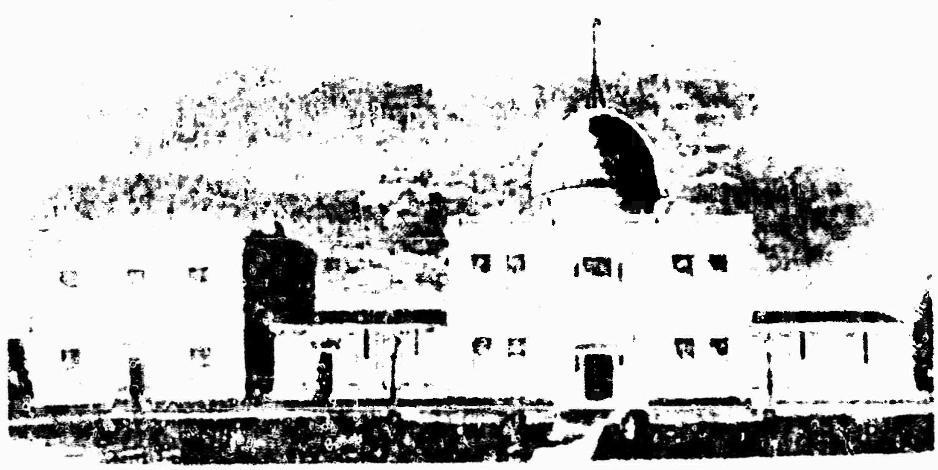
The foundations laid from 1830 to 1865 are still apparent today. The contemporary U. S. Naval Observatory carries out transit circle observations to determine the fundamental celestial coordinate system, employs sophisticated atomic clocks and photographic zenith tubes to maintain its time service, and prepares *The Astronomical Almanac* (formerly *The American Ephemeris and Nautical Almanac*) aided by the latest electronic computers.

There is also a long-term program of double star and parallax measurements, both with the 26-inch refractor and with the 61-inch astrometric reflector in Flagstaff, Arizona. A new program in radio astrometry is opening yet another phase. The Naval Observatory continues to play an important role in the growth of American astronomy and in the development of positional astronomy, timekeeping and navigation throughout the world.

The author thanks Robert W. Rhynsburger, Theodore J. Rafferty and Brenda G. Corbin of the Naval Observatory for their assistance in preparing this article.

NOTES

1. National Archives, Record Group 45, Entry 214, "Letters to Officers."
2. National Archives, Record Group 45, Entry 213, 27 June, 1831, "Letters to the Secretary of the Navy."
3. National Archives, Record Group 45, Entry 228, 10 June, 1831. "Letters from the Depot of Charts and Instruments."
4. Ibid., June 19, 1833.
5. Gilliss, J. M., *Astronomical Observations made at the Naval Observatory, Washington, under Orders of the Honorable Secretary of the Navy*, dated August 13, 1838 (Washington, 1846), p. x.
6. Ibid., p. xiii.
7. National Archives, Record Group 45, Entry 228, "Letters from the Depot of Charts and Instruments."
8. Report of the Secretary of the Navy, communicating a "Report of the Plan and Construction of the Depot of Charts and Instruments, with a Description of the Instruments," Senate Document 114, 28th Congress, 2d session, p. 114 (by J. M. Gilliss).
9. Gilliss to Elias Loomis, 18 October, 1844, transcribed in Nathan Reingold, *Science in Nineteenth Century America* (New York, 1964), p. 138.
10. *Astronomical Observations made under the Direction of M. F. Maury, Lieut. U. S. Navy, during the year 1845, at the U. S. Naval Observatory, Washington* (Washington, 1846), Appendix, p. 118.



The Naval Observatory after the addition of the superintendent's residence in 1847–48. This engraving is taken from a guidebook to Washington, courtesy of R. L. Sch[illegible]

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